

37-6-19

MISCELLANEOUS
EXPERIMENTS AND REMARKS
ON
ELECTRICITY,
THE
AIR-PUMP,
AND THE
BAROMETER:

WITH THE DESCRIPTION OF AN
ELECTROMETER OF A NEW CONSTRUCTION:

ILLUSTRATED WITH COPPER-PLATES AND NOTES.

By *A. BROOK* *K*

Or by collision of two bodies grind
The air attrite to fire, as late the clouds
Jostling or pushed with winds, rude in their shock,
Tine the flant lightning, whose thwart flame driv'n down
Kindles the gummy bark of fir.

NORWICH:

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MDCCLXXXIX.

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MISCELLANEOUS

EXPERIMENTS AND REMARKS

ON

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MISCELLANEOUS

ADVERTISEMENT.

THE Electrometer mentioned in the title-page has been published in the *Philosophical Transactions*, Vol. LXXII, but by some means not generally known, and the omission there of many necessary references (although the same drawings of the plates were in the hands of the compilers of that very useful and extensive publication) it is not easy to discover the mechanism, nor the method of using the instrument; from which it was thought, that a republication, and a more accurate description thereof, might make it better understood, so that the workmanship may be more easily executed, and the instrument be used without difficulty: and if the language, in which the following sheets are written, can be understood, the Author will obtain what he aimed at, having no great pretensions to erudition, and meaning only to relate plain facts.

NORWICH,
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A 2

INTRODUCTION.

AS so many excellent pieces, by way of introduction to electricity, have already been written, what is contained in the following sheets is not so much intended for the instruction of beginners in that branch of natural philosophy, as to find out some way of admeasurement thereof, so as to be generally understood; or in such a manner, that one person may communicate experiments to another, at any distance of time or place, by such a measure, as that they may both succeed alike. Accordingly, after having described the electrometer, &c. represented by the copper-plates, by which such an admeasurement is to be ascertained, a course of experiments is set down in the promiscuous order they were made. The former part of them, to about Experiment

periment XXIX. were trials with different numbers of bottles, charged to different heights, on wires of different metals, sizes, and lengths, in order to find out the strength of the charge of a given quantity of coated glass necessary to produce the effect proposed. The latter, are experiments, where those charges are reduced more to a certainty, either with positive or negative electricity, so as to know, before-hand, what strength of charge is necessary to melt a wire of either lead, iron, brass, or copper, of a given length and size; which will tend to point out pretty nearly (where bell-wires have been melted by a thunder stroke, the size, sort of metal, and the length of the wires that were destroyed being known) the strength of the stroke from the cloud where such accidents have happened; and thence to find what quantity of coated surface is necessary, and to what height it must be charged, in order to do the same.

The battery with which some of the following experiments were made, consisted at first of
only

only nine jars, or rather bottles, for they were not with open mouths; but when a greater power was wanted, it was necessary to procure more bottles, or jars; but the latter not being easily procured, the former were substituted in lieu of them; though afterwards a much less number of bottles was used, solely for trial sake. The mean size of these nine bottles was eight inches diameter, coated about ten inches high, of the thickest and strongest glass that could be procured*, weighing from five and a half, to about seven and a half pounds each. The bottles which composed the battery after Exp. XXXII, were disposed in three rows, nine in each row; nine of the stoutest and best composed the first row, nine of the next in stoutness made the second row, and the third row contained the nine weakest, (of which notice will be taken in the work) all of green glass, but not all of one sort of green glass. Some of them, which stood foremost in the battery,

* See Remark IX.

were

were made of a sort of glass very much like that of the Frontinac wine bottles. The glass of which those bottles are made seems to be by much the best, being much harder and stronger, and seems to bear the highest charge without being struck through. One of these wine bottles which is three inches and a half diameter, and coated seven inches high, has often been charged to thirty grains repulsion (between two balls of the size of those on this new constructed electrometer) and has not been struck through.

The second and third rows in the battery, consisted of bottles from about six and a half, to ten inches diameter, coated from eight and a half to eleven inches high; and none of their mouths are more than one inch and a half, nor less than three quarters of an inch wide, in all the battery. The highest charges being made with bottles apparently the strongest, if any of them were struck through, the fracture was repaired as in Experiment XLIII, and removed to a lower order, when a deficiency

iciency happened there, as will be observed in the work: and when any measure of coated glass or surface is mentioned, it is meant only of external coating.

Notwithstanding the above battery consists of such *narrow* mouthed bottles, on account of *wider* not being easily procured; yet, the latter are much preferable, being easier to coat; moreover, the electricity is not so liable to be dispersed by the wires that go into the bottles when their mouths are wider; for if particular care be not taken, (and even this will hardly be sufficient) the electricity will frequently be spurting from the sides of the wire, to the edges of the mouths of the bottles, when they are thus narrow, without bringing on a spontaneous discharge. This being the case with the Frontiniac wine bottles, I have never been able to charge them so high as they would bear: therefore I cannot tell how much more, than to thirty grains, they might be charged, without being struck through: but by the time that the charge
a gets.

gets to be thus high, the spurting will disperse the fluid as fast as the machine will collect it. The number of bottles that have been destroyed in making the following experiments, has, in several of them, made a small difference in the result; but could the same battery have been preserved entire, throughout the making of these experiments, I believe there would have been so little difference, as not to be of any material consequence, and still less difference, if wires perfectly homogeneous, could have been procured. This difference appears most in the brass wire (at the former part of the experiments, when my stock of brass-wire was very low, and I was never able to procure any more like it) with regard to its stretching so much while hot, which, to me, appeared very singular.

As wires of exactly the same size may be rather difficult to procure at all times, and lest a method to prove them so, may not be always at hand, the method I took to prove what those wires were, both in weight and
size

size, that I used in the following experiments, is here subjoined. Each piece of wire that was weighed was cut ten inches and a quarter long; as every piece of wire that I tried was the length specified in the experiment between the forceps with which it was held. I have also added the weight of a piece of each wire, both of their sizes, and the sort of metal: every piece of wire that was weighed was the same in length. The weight of them is added to be a check upon the measure of their thickness; and the scales were such as turned freely with the two hundred and fifty-sixth part of a grain. In order to get the thickness of each wire, as near as possible, they were wound round a wire one-tenth of an inch thick, on which an inch space was carefully marked; the revolutions of the wires being kept as close as possible to each other and their number being known, gives the thickness of each wire, pretty nearly: the iron and brass wire was such as is used for harpsichords, &c.

All my battery, as well as all my other bottles, or jars, both withinside and without, is coated with strips, cut from three-eighths to three-fourths of an inch wide, and laid on with paste made with flour and water, at the distance of about a strip between each.

The following table exhibits the sizes and weights of various wires of four different sorts of metal.

Parts

INTRODUCTION.

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Parts of an Inch in thickness.	Weight of each in Grains and Parts of a Grain, 10 $\frac{1}{4}$ Inches long.	Proportion to each other.
Lead Wire.		
66th	5 grains and $\frac{3}{4}$ of a grain	above half the 50th.
56th	7 grains and $\frac{1}{6}$ of a grain	
50th	9 grains and $\frac{1}{4}$ of a grain	nearly double the 66th.
Iron, or Steel Wire.		
170th	$\frac{1}{4}$ of a grain	
140th	$\frac{3}{8}$ of a grain	almost half that of 100th.
120th	$\frac{1}{2}$ of a grain	
100th	1 grain and $\frac{9}{16}$ of a grain	more than double 170th.
90th	1 grain and $\frac{5}{8}$ of a grain	
80th	2 grains and $\frac{1}{4}$ of a grain	almost double 100th.
64th	3 grains and $\frac{7}{16}$ of a grain	
56th	4 grains and $\frac{5}{8}$ of a grain	{ more than double 80th, or almost four times 100th.
Brass Wire.		
170th	$\frac{2}{3}$ of a grain	
120th	1 grain and $\frac{1}{4}$ of a grain	almost one-sixth part of 50th.
64th	5 grains and $\frac{3}{8}$ of a grain	
50th	6 grains and $\frac{1}{16}$ of a grain	more than six times 170th.
Copper Wire.		
170th	$\frac{1}{16}$ of a grain	
140th	$\frac{1}{10}$ of a grain	
130th	$\frac{1}{11}$ of a grain	about $\frac{1}{3}$ of 140th, which is equal against the electrical stroke as 100th of steel wire, as appears by the following experiments.

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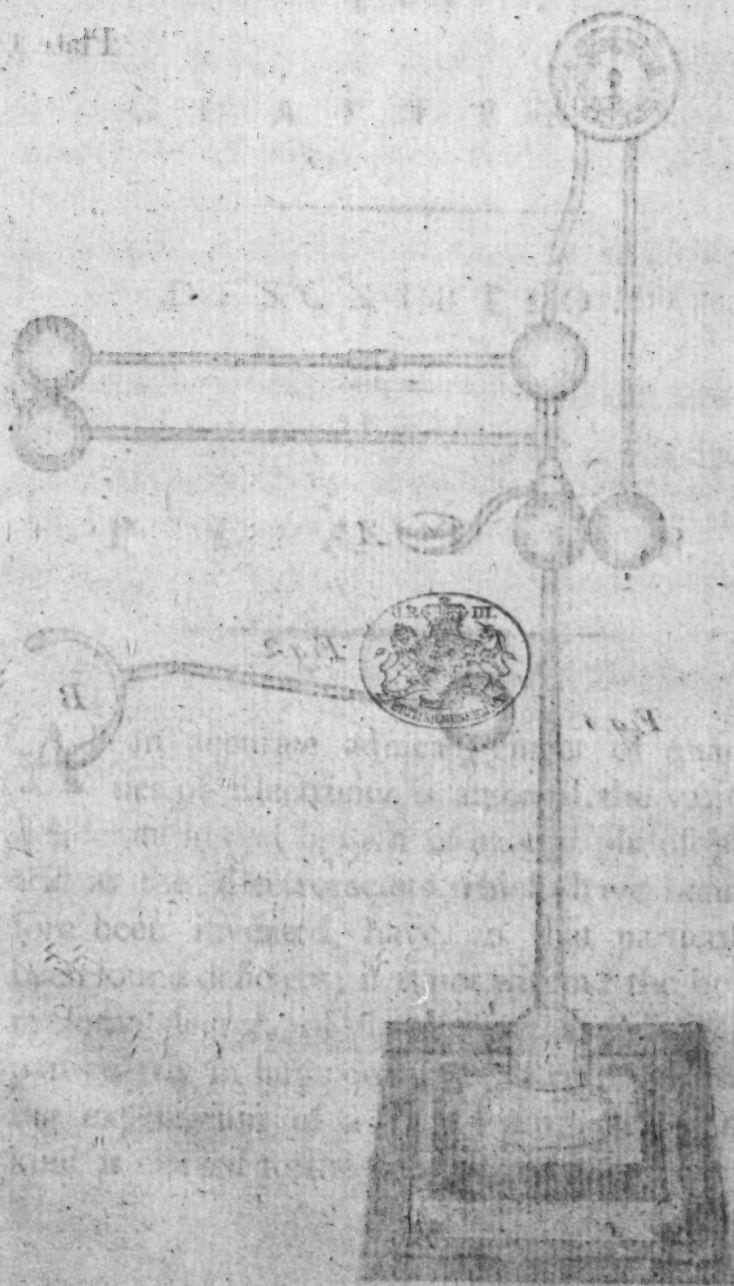
CHAPTER III.

E R R A T A.

PAGE 36, line 6, read *nearly* instantaneously.

— 86, last line but one, read *or* as in.

— 89, line 15, for 63, read 81 and 82.



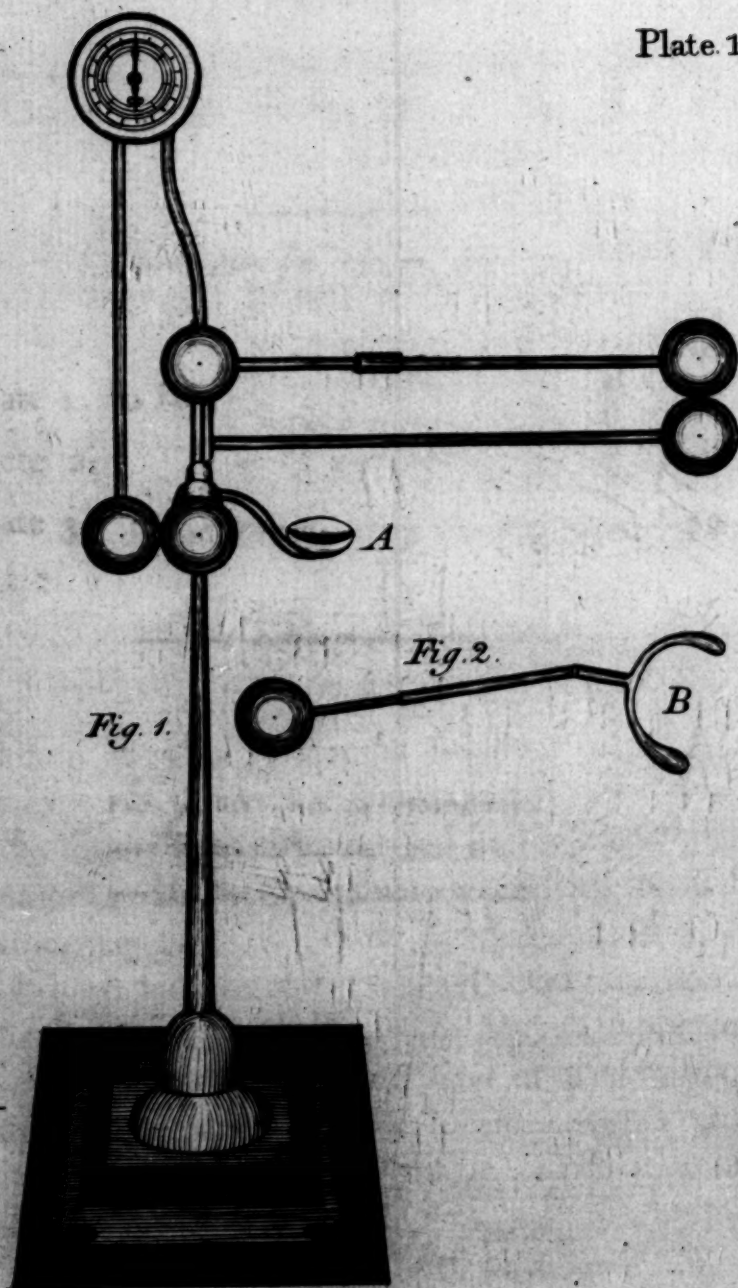


Plate 1. shows the Electrometer
C H A P T E R I

to be used. This is a small instrument, the ball of which is to be laid into the cup, &c. to be explained
 maintained to a battery, &c. to be explained

D E S C R I P T I O N

Plate 2. exhibits the outlines of all the
 of the
 Plate 3. exhibits the outlines of all the
 of the
P L A T E S.

AS an accurate admeasurement of quantities of Electricity is amongst the various desiderata in that branch of natural philosophy, and as the Electrometers which have heretofore been invented, have, in that particular, been found deficient; it is not without the hope, in some degree, of supplying that deficiency, particularly in large quantities, that the following explanation of a new instrument of this kind is offered to the public.

B Plate

2 DESCRIPTION OF THE PLATES.

Plate 1, fig. 1, shews the Electrometer in miniature, as it appears when it is ready to be used. Fig. 2, is an arm, the ball of which is to be laid into the cup *A*, to make a communication to a battery, &c. to be explained afterward.

Plate 2, exhibits the outlines of all the different parts, both internal and external, of the Electrometer, in full size and proportion, and the manner in which they are made and put together.

A, A, A, A, N, fig. 1, and fig. 1, *a*, represent the Electrometer in full size all put together; except where fig. 1, *a* is separated from fig. 1.

The foot *B*, fig. 1, *a**, is a piece of board, nine inches and three quarters square, resting on three pins *C C c*, seen at the under side of it. The pins *C C*, with their broad heads, are

* This figure is wholly omitted in the Philosophical Transactions, although it is referred to.

screws

screws to set the Electrometer upright withal. *D*, is a solid piece of glass, of any length and size, sufficient to support and insulate the instrument, wherever it is to be placed: the length of that here described is about nine inches and a half between the sockets in which it is fixed, and its upper end is seen in the socket, or cap *M*, fig. 1.

The arms *G* 1, and *G* 2, and the balls *I* 1, and *I* 2, fig. 1, are all made hollow, of thin copper, that they may be as light as possible. The arms *G* 1, and *g*, with the ball *F*, turn round on the large bent wire *H*; and when in use they are put nearly at a right angle with *G* 2, and *H*; being all turned to the off side, so as to be, as much as possible, out of each other's atmospheres, or the atmosphere of a jar, battery, prime conductor, or the like. At *K*, is represented a kind of face, or dial plate, with an index; this index is carried once round by the motion of the arm *G* 2, with its ball *I* 2, moving through a quarter, or ninety degrees of a circle; the motion is given to the arm and its ball, by the repulsive power of the

B 2 charge

4 DESCRIPTION OF THE PLATES.

charge of a jar, &c. between the two balls *I* 2, and *L*. The ends of the index, from the center, are of different lengths; the longest end reaches to a graduated circle, divided into ninety equal parts, answering to the ninety degrees, which the arm *G* 2 moves through: the shortest end reaches to a smaller circle, which may be divided into any number of equal parts answering to the divisions on the arm *G* 1, with its sliding weight *n*, each of which, is equal to one grain, and the whole face is covered with a watch glass*, to prevent the electricity from flying off, or escaping at the points.

The upper end of the glass-supporter *D*, is cemented into the brass cap, or socket *M*, fig. 1. This cap enters the ball *L*, at the bottom of it, and is screwed into its upper part at *a*. The top part of the cap *M*, is tapered off to

* There was an Electrometer constructed by a very ingenious Gentleman some years ago, the face of which was covered with a glass: but it seemed to me to be less useful than Henley's Electrometer, though much more complex; yet its neatness was very pleasing.

DESCRIPTION OF THE PLATES. 5

a cone about one inch and a half long. The lower end of the wire *H*, has a hole made conically into it, so as to receive the upper part, or conical end of the cap *M*, which permits all the upper part of the Electrometer to turn round any way that may be necessary. The kind of ferrel *O*, with its base, is perforated, that the lower end of the wire *H*, may go through it. The arm *b*, which supports the cup *N*, is screwed into the base of the ferrel *O*, and turns round upon the wire *H*. The cup *N* is to receive the ball *P*, of the arm fig. 9. This arm, which is shewn separate in plate 1, fig. 2, shortens or lengthens as may be wanted, by a wire sliding into a tube; the end of the wire is slit, forming a spring in the tube, to give it steadiness. In this arm is a kind of rule joint at *d*, that it may give way easily if wanted: the semicircular end of the arm is a spring, which may be slipped on to a ball from the prime conductor, or the conductor itself, (if it fit) or jar, or battery: the ends of it are flat and broad, as represented at *B*, fig. 2, plate 1.

Fig.

6 DESCRIPTION OF THE PLATES.

Fig. 2, shews the wheels &c. in the Electrometer*. The wheel *A*, has forty-eight teeth, and takes into the wheel *B*, of twelve teeth. The axis of *B*, carries the index *K*, fig. 1, once round, by the arm *C*, fig. 2, being repelled to *D*, a quarter, or ninety degrees of a circle, as before observed. The arm *C*, is screwed into the underside of the axis of the wheel of forty-eight teeth. *E*, is a solid leaden weight, supported and made fast to the upper side of the same axis; this leaden weight, which is almost a counterbalance to the arm *G* 2, and its ball *I* 2, fig. 1, may be made of any shape, so that it be heavy enough, and move freely within the Electrometer. At *F*, fig. 2, is shewn a circular piece of brass, fixed within side, so as that the repulsive power, at the top of the leaden weight *E*, may be as equal as possible, at whatever distance the arm *C* is repelled. At *G*, is shewn the piece of brass into which the upper end of the wire *H*, fig. 1,

* All the references and descriptions of fig. 2, to fig. 11, are entirely left out in the Philosophical Transactions, and only fig. 3, fig. 4, and fig. 10, are kept the full size, although otherwise expressed. All the other figures are much diminished.

is screwed, it is made fast to the inside of the frame of the instrument with a screw.

Fig. 3, shews the internal frame of the Electrometer, to which all the parts are fixed and supported by screws, &c.

Fig. 4, shews the inside of the Electrometer in profile. The brass rim *A*, which holds the glass in the front face of the instrument, is kept in its place by center-pins at *c c*, and is held on by the screws *a a*. The part *B*, which brings the dial plate forward from the frame towards the glass, is held on by the screws *bb*. The axis of the wheel of twelve teeth passes through the dial plate on *B*, and receives the index *k*; the other end of the axis is pointed, and works in the bent arm *d*, which is screwed to the frame at *e*. One end of the axis of the wheel of forty-eight teeth is pointed, and works in the frame; the other end is supported by, and works in, the bent arm *g*, which is screwed to the frame at *h*; but to lessen the friction at *i*, in the bent arm *g*, a screw

8 DESCRIPTION OF THE PLATES.

screw passes through it; this screw is perforated, as at fig. 10, (represented enlarged) by this means, such exactness in the length of the arm *g*, is unnecessary, the screw itself being perforated with a larger hole till very near the little end of it, which is bevelled off, and the remaining part of the hole being made very small, the friction is considerably diminished. The joinings of the weight *E*, and the arm *C*, are shewn here, the same as in fig. 2. The part *D*, which brings the dial plate on the back of the Electrometer towards the glass, rests in the rabbet of the frame, and rises half the depth of the rabbet. The brass rim *F*, which holds the other glass, rests in the same rabbet of the frame, fills up the other half of the rabbet, and is held on by a bent pin, as at *o*, on one side, and by a spring catch *p*, on the other side; this bent pin, and the spring catch pass through two holes (under, or behind the brass rim *F*,) made for that purpose in the part *D*. The axis of the wheel of forty-eight teeth passes through the dial plate *D*, and carries the index *m* a quarter,

or

DESCRIPTION OF THE PLATES. 9

or ninety degrees of a circle, which may be graduated answerably to the face on the front of the Electrometer.

Fig. 5, represents the Electrometer turned its bottom upward, to shew the opening, where the arm *G* 2, fig. 1, moves up and down, and the hole through which goes the upper end of the wire *H*, fig. 1, and screws into *G*, fig. 2. The catch *p*, which is shewn in fig. 4, is fixed to the frame, close to the opening in fig. 5, that it may be opened easily with the point of an awl, or the like.

Fig. 6, fig. 7, fig. 8, fig. 11, shew the mechanism of the movements and parts in the ball *F*, fig. 1.

Fig. 8, shews the external construction of the ball *F*, fig. 1, which is hollow, and consists of a frame, or rim, in the middle, with two caps screwed on to the rim, one on each side; it has a hole at top and bottom, through which goes the bent wire *H*, fig. 1. Within

C

this

this ball is another leaden weight, which is a counterbalance to the arm *G* 1, fig. 1. The innermost circle in fig. 8, represents the outline of the weight in the ball *F*, fig. 1. The position of this weight is seen at *g*, fig. 6, and *g*, fig. 7; a hole is made perpendicularly through the middle of it, to give liberty for it to move up and down, free of the wire *H*, fig. 1, which passes through this weight.

Fig. 11, shews a piece of brass, a little thicker than a new shilling, which is held to the inside of the rim, fig. 8, by the screws *a a*, fig. 6, with its hole in the middle, against the hole in the rim fig. 8, in which the arm *G* 1, fig. 1, moves a little way up and down, as shewn in fig. 6. The ends *b b*, fig. 11, are turned up as at *b b*, fig. 7, to form a supporter for the axis on which the arm *G* 1, fig. 1, with its weight *g*, fig. 7, rests. *a a*, fig. 11, shew the holes, through which go the screws *a a*, fig. 6. By these four figures the whole construction is shewn better than by a whole page in letter-press. This weight is made fast to the

the axis on which it is supported; but the arm G 1, fig. 1, screws in and out as at c fig. 6.

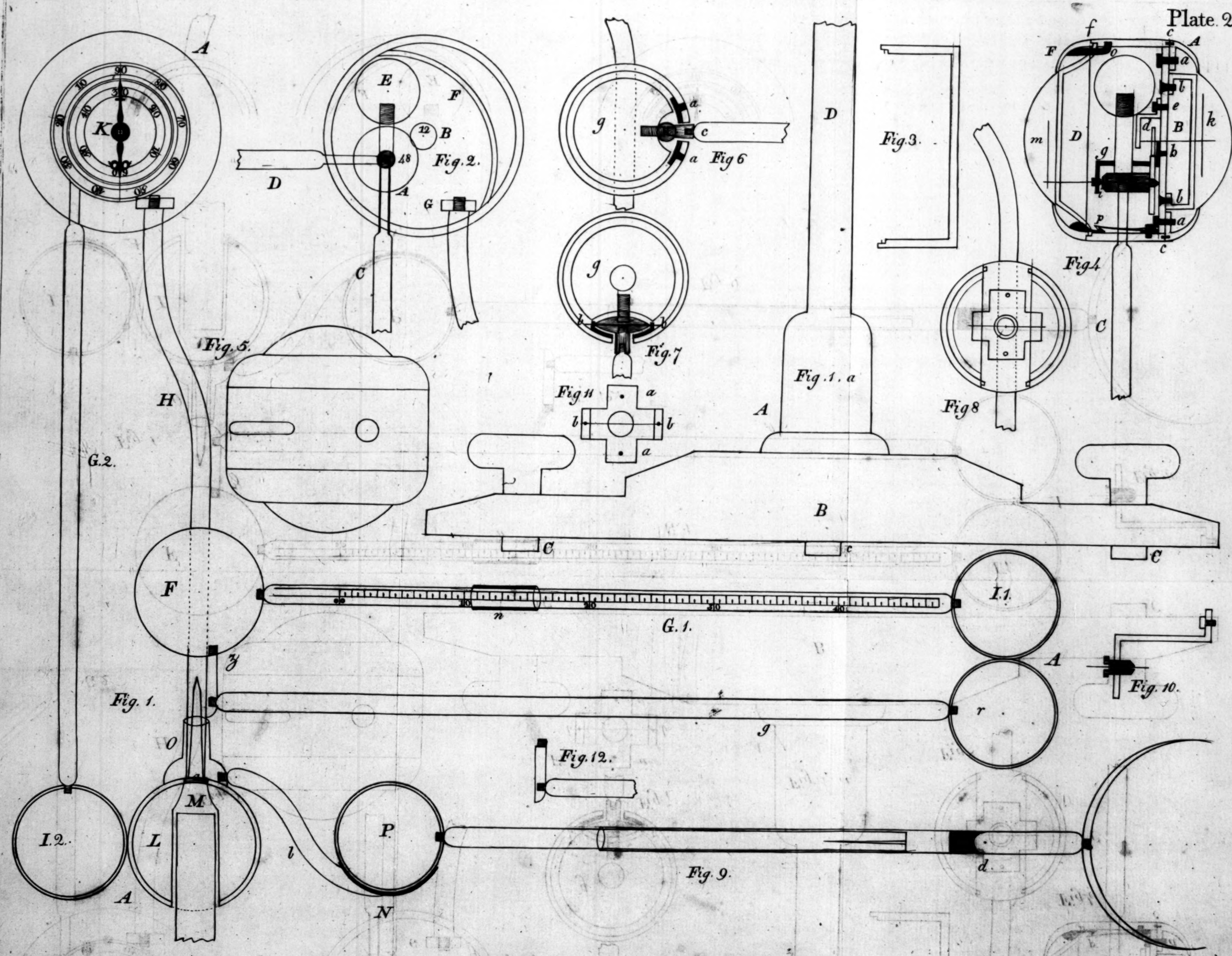
Fig. 12, shews the part at z , fig. 1, that screws into the ball F , to support the arm g , and its ball r : this piece (which is hollowed out on the side next the wire H , to fit it) is screwed in, so as to press against the wire H , and serves as a spring to keep the ball F steady; which is made to slide up and down, as well as to turn round on H , to whatever position may be wanted.

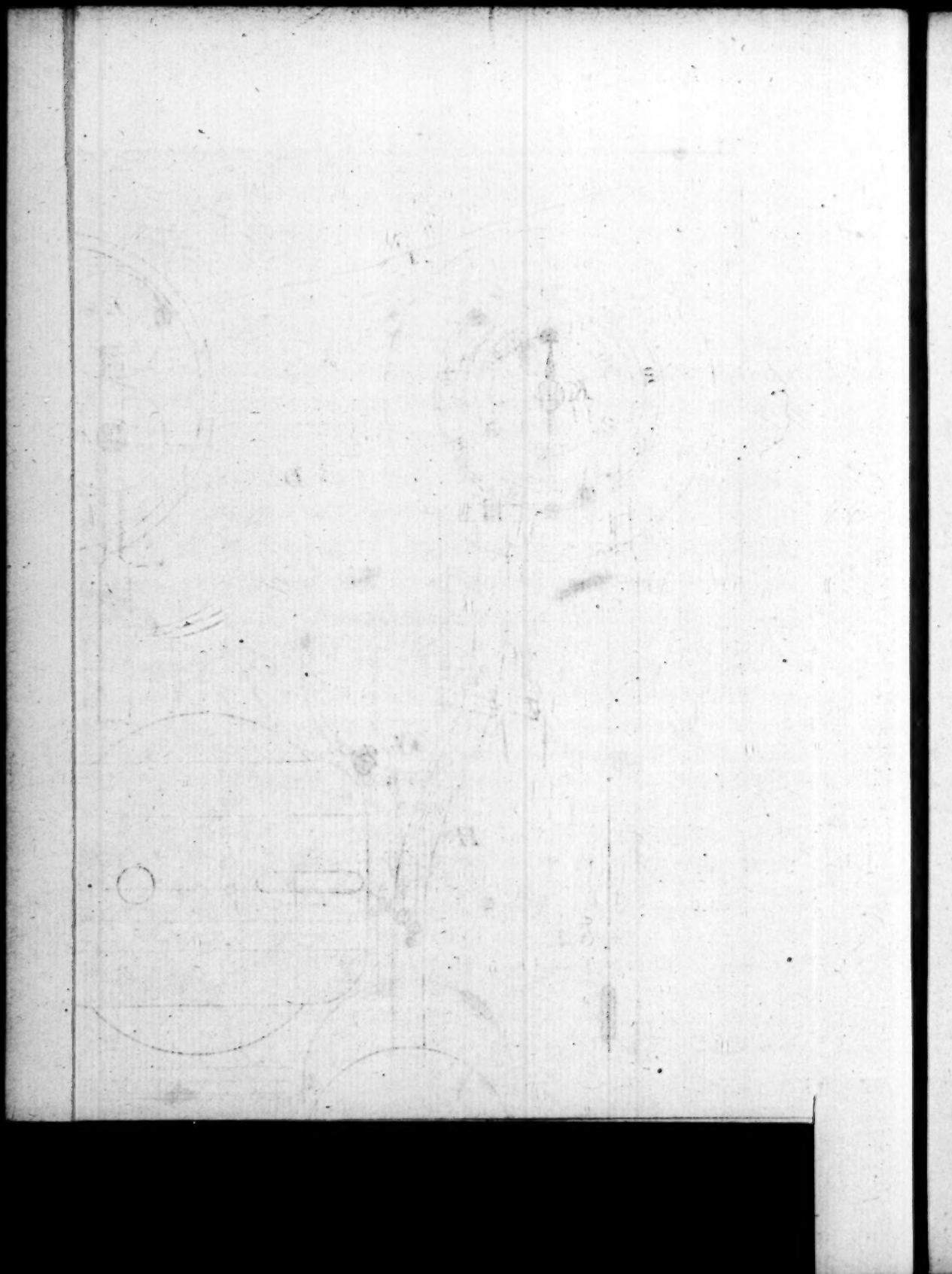
In order that the divisions, before-mentioned, on the arm G 1, fig. 1, may be made exactly a grain each, first slide the weight n , towards the ball F , till it is an exact counterbalance to the weight in F ; then at one end of the weight n , make a mark on the arm G 1, and there let the divisions and numbering begin. Suspend any tolerably good pair of scales, so as that the bottom of one of them may rest upon the ball r , between r and I 1, (the ball I 1, resting in the scale) slide the weight n , near to the ball I 1,

put as many grains into the other scale, as will just raise the scale with the ball *I* 1, in it; then mark the arm at the same end of the weight *n*, and divide the space between the two marks, into as many equal parts as there are grains in the scale; each part will then be equal to one grain: each part may also be divided and subdivided into halves and quarters.

The arm *G* 2, fig. 1, being repelled, shews when the charge is increasing, &c. and the arm *I* 1, with its weight *n*, tells what such repulsive power is between two balls of the size of these, in grains, according to the number of divisions, at which the weight *n*, on the arm *G* 1, rests, when it is lifted up. The weight *n*, by repeated trials, having been put at different places, such respective number of grains may then be marked on the lesser circle on the dial-plate, to which the shortest end of the index *K*, fig. 1, reaches, and where it points at*:
 so

* The Gentleman who invented the Electrometer, mentioned in the foregoing note, constructed it so that the index was carried twice





so that when the grains are all marked on the dial-plate, thus ascertained by the arm *G* 1, all these parts of the instrument, that is, the ball *F*, with the arms *G* 1, and *g*, may be taken away, and the remaining part of the Electrometer may be used without them: but I do not know how the grains can be so exactly marked and ascertained on the dial-plate, as by these parts being on the instrument. And, notwithstanding these parts may be taken off, and the Electrometer used without them, yet I think it is most safe and certain, to use it with them altogether, as they are a check one upon the other: for I find various circumstances will occur, by which

twice round the dial-plate, and he told me that a friend of his was going to make one with the index to go four times round, but I think it must be quite unnecessary, and not so distinct: also, that the ascertaining, and marking the grains on the dial-plate, as well as the number of degrees the moving arm is repelled, must be very indistinct. But his instrument (which was the first that I ever heard of, where the wheel-work was introduced) was not constructed so as, in any degree, to shew what the repulsive power was, any more than that of Mr. Henly's construction. In order to have as much distinctness as possible, I made the index to go only once round the dial-plate, by which means both ends of the index become useful and intelligible.

the

14 DESCRIPTION OF THE PLATES.

the arm G 2, alone is more liable to vary at different times, than when the other parts are on: the atmospheres of different things, or the atmosphere of the arm and its ball, interfering with other things, or any small piece of lint, or other fibrous substances, lighting on the ball, will make it vary very much, according to the place it may, by chance, adhere to: but, as before observed, when they are all together they check each other. I believe the arm G 2, alone, would be equally certain, if all imperceptible atoms, &c. could be got rid of which are constantly floating about in the air of most rooms.

I do not mean to confine the number of grains, or divisions on the arm G 1; but my own observations rather lead me to think, that no glass to be charged, as it is called, with electricity, will bear a greater charge than that whose repulsive force, between two balls of the size of these, is equal to sixty grains weight, before it will be perforated, or struck through. Nay, I have not found many instances when it would bear, or support itself
with

with fifty grains; and I think it very hazardous to charge higher than forty-five grains. I think, likewise, that the size of these balls and wires are large enough to prevent the escape, or dispersing of the charge of electricity, which any glass will bear, that we are acquainted with; but if balls, wires, or arms, of this size, are found too small, larger may be made on the same plan.

After this manner, with the Electrometer above described, by knowing the quantity of coated surface, and the diameter of the balls, one may say, a certain quantity of coated surface of glass, charged to any given number of grains, expressing the repulsive force, between two balls of a certain diameter, will melt a wire of a certain size, kill this or that animal, and the like.

In respect to the uses and advantages of this Electrometer, perhaps I may not know them all, and left partiality may prejudice me in behalf of my own contrivance, I would rather leave them to the judgment of others: my
 opinion

opinion however is, that all others, which I have seen or heard of, are such as speak no intelligible language, and that this speaks so as to be understood universally: for unless the repulsive power of the charge of different glasses be very different, this Electrometer, or any other Electrometer, made after this manner, must, I should presume, speak very nearly the same language; it being known how much coated surface there is, and the size of the balls: but if the size of the balls be not the same, the language the Electrometer speaks, will be very different.

Although other Electrometers shewed a greater or less charge, or power, by an arm being repelled to a greater or less distance, or by striking at different distances; yet, the power of the charge, was not in any manner ascertained; we could say, that the arm, or the index, was repelled to such a number of degrees of a circle, or that it struck at a certain distance; but the repulsive power of a charge to repel the index so much, or so many degrees of a circle; or the strength of the charge to strike

at such a distance, was not, that I know of, in any manner intelligibly ascertained: this does it by the weight that the repulsive power has to lift up in grains, &c. which weight is to be proved by any good pair of scales and weights; and I know not of any method that has yet been tried to shew the different strength of charges, so satisfactory as by that of their repulsive force.

All the necessary parts of the Electrometer being made of metal and glass that is pretty stout, I think that the electricity is considerably less liable to escape than by wood, &c. I have tried reeds, on account of their being light, and covered them with tin-foil, or gilded them, to make them good conductors, but have so frequently found inconveniencies from them by points rising up, the celerity of moving, and the different weight of them at different times, owing to moisture, changes of the weather, and the like, that I have laid them all aside, and find my present instrument as free from these inconveniencies as I could

D

expect;

18 DESCRIPTION OF THE PLATES.

expect; nor is it liable to be out of order, if proper care be taken of it.

Another use of this Electrometer is, to graduate other electrometers; I mean such as Mr. Nairne's, which I think is a very good improvement of Mr. Lane's electrometer; and I think I have rather improved Mr. Nairne's, by a little alteration in the construction of it, and graduating it, so as to be always certain; and it may be applied to any prime conductor, &c. but unless it be first graduated, that the strength of the charge may be ascertained by the distance it is to strike at, it is of no more use than Mr. Nairne's; except that it is applicable to any thing, or almost any where; yet, after all this alteration, it will never shew when a charge is increasing or decreasing, nor how far off; or how near a discharge is, but only tell what it is, at the time that the discharge is made.

Although the electrometer described in plate 1, and plate 2, has the advantages and uses

uses already mentioned, yet, it will never make the discharge of a jar, or battery, of itself, for which Mr. Nairne's is very excellent: and in the state to which I have altered it, I think it is exceedingly useful with the former one; thus, the progress of a charge is shewn by the motion of the arm G 2, of the former; the distance it is repelled shews how far off, or how near, a discharge, of any required strength is; the arm G 1, will shew that that repulsive power is what it was designed to be; and the latter, will make the discharge, as soon as the charge has acquired the strength proposed: which I have proved, having one to apply to the machine that was used in making the following experiments.

Plate 3, fig. 4, shews this last mentioned electrometer in its improved state. A, is a solid piece of glass bent to a right angle at both ends; one end of it goes into a socket cut in the ball B, which is of solid metal, to one side of which is made fast a short conical tube of tin. The prime conductor has likewise a conical tube, or socket, fixed in

the side or end of it, to receive the conical tube fixed to the ball *B*: they are made conical that they may stand steady. The other end of the glass *A*, goes into the socket of wood *C*, at the end of which is a ball; through this ball goes the graduated wire *D*, one end of which terminates in a ring or with a ball: the other end has another ball on it, equal in size to the ball *B*, both of which are the size of the balls of the electrometer described in plates 1 and 2. The socket cut in the ball *B*, being concentric to the ball itself, the glass *A*, will turn about in it; still the ball on the graduated wire *D*, will always be at the same distance from the ball *B*.

Notwithstanding all the improvements and alterations that have been made on Mr. Lane's electrometer, both as to acquiring charges of exactly the same strength with the same quantity of coated surface, and as answering the end, or use, of a discharging rod, still, with respect to the latter, it is chiefly useful, when the charges are not very high, nor a great quantity
of

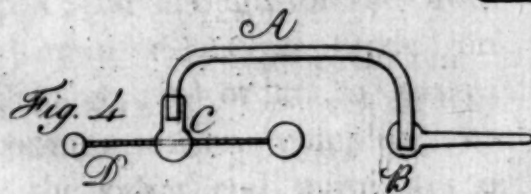
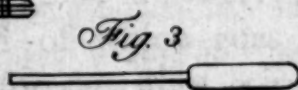
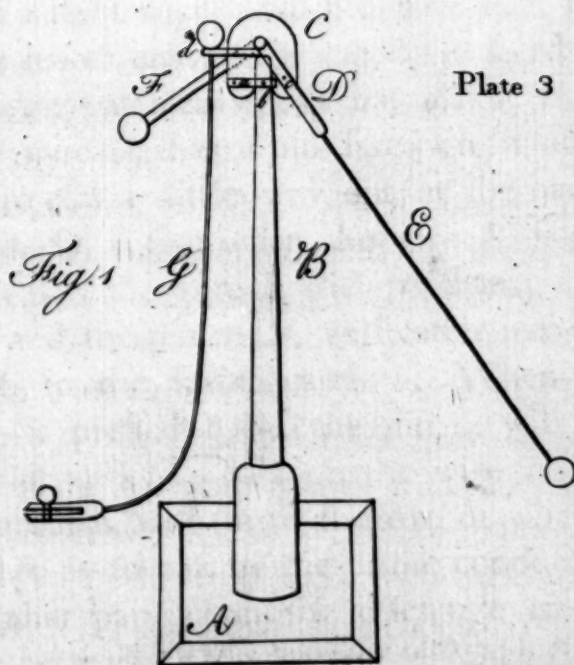
of coated surface; many fibrous particles (constantly and almost imperceptibly floating in the air of a room) being so apt to be attracted by, and adhere between, the balls of the electrometer; and the electric atmosphere extending, so strongly, much farther than the distance of these two balls; so that, in both these cases, the charge is very liable to be diminished, almost imperceptibly, whilst the battery is charging. Therefore, with large batteries, highly charged, a discharging rod, or a contrivance for that purpose, is much better by itself, entirely detached from the machine, or the prime conductor; and in such a manner, that it may always approach the place, where the discharge is to be made, with the same velocity, and at a certain distance. For this purpose, it is easily made to do its office either with a spring, or a weight, fixed to one end of a wire, and a light ball at the other end of it, with a spring catch, to be made to let go its hold, when the discharge of a battery is wanted. Such an one is adapted to the present machine, and is fixed, by a cramp-iron and screw, upon the table on which the electrical

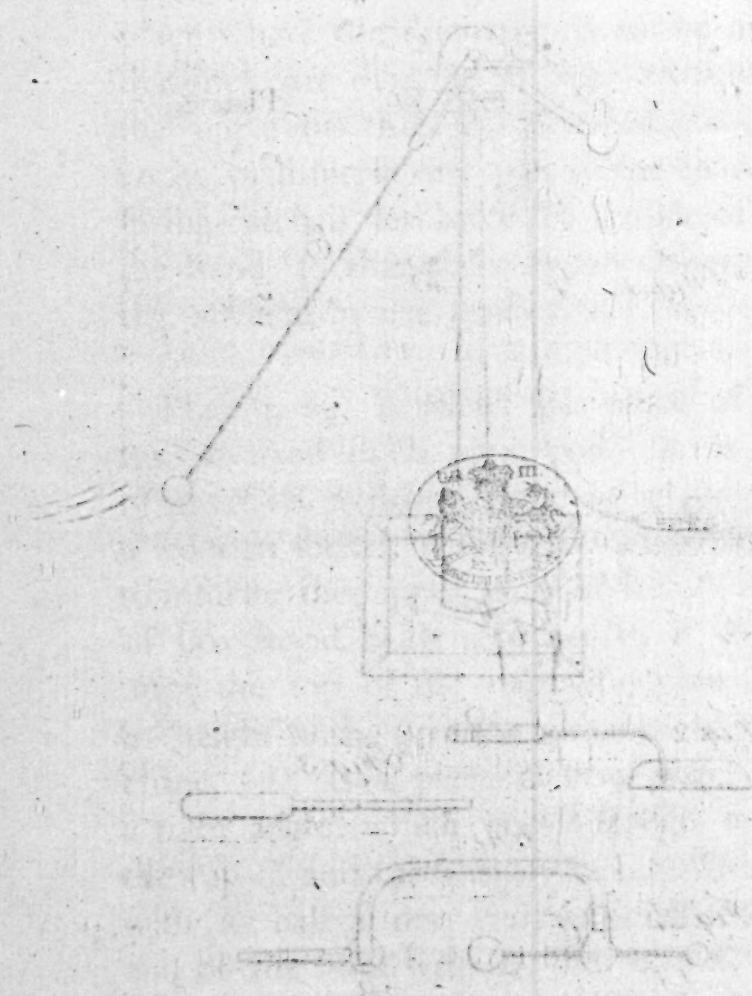
22 DESCRIPTION OF THE PLATES.

trical apparatus is placed, at the distance of about sixteen or eighteen inches from the place where the discharge is to be made, so that no part of it be so near to any part of the apparatus that is electrified, as to steal away, or disperse any part of the charge. The spring-catch is let loose by a stick of glass in the hand, so that there is no danger of being affected by the stroke.

Plate 3, fig. 1, shews this piece of apparatus, or fixed discharging rod. *A*, is the foot of it. *B*, is a strong piece of glass, fixed in a wooden socket, which screws into the foot *A*, to insulate the upper part of it. *C*, is a cap of box-wood, with a socket in it to be put upon the top of the insulating glass *B*, equal in height to the prime conductor of the machine. *D*, is a piece of stout wire, bent to a right angle, which goes through a hole in the cap *C*, and turns freely in it. The wire *E*, with its ball at one end, is screwed into the end of the stout wire *D*; the wire *E*, together with its ball, is about sixteen inches long. The wire *F*, has a ball of lead fixed to one end

Plate 3





end of it, the other end is fixed to the end of the stout wire *D*, at the back of the cap *C*, nearly at a right angle with the wire *E*. Towards the lower part of the cap *C*, is fixed to it, the spring-catch *a*, which has a hole in it at *b*; the wire *D*, has a pin fixed in it at *c*, which projects a little way out at the back-part of it, so as that when the wire *E*, is put perpendicular, the wire *F*, will be nearly horizontal, and the pin in *D*, will catch into the hole at *b*, in the spring catch *a*. When the spring *a*, is pressed back, the pin *c*, will let go its hold, and the ball on the wire *F*, falling downwards, will throw the arm, or wire *E*, upwards, so as to pass by the prime conductor, or any other part where the discharge is designed to be made, at a certain distance from it, and make the discharge of a battery, &c. At *d*, is a wire, one end of which goes into the cap *C*, far enough for the stout wire *D*, to rest upon it as it turns, on making the discharge, in order to make the metallic communication complete through the cap *C*: the other end terminates in a ring: through this ring goes the wire *G*, with the

the ball on its upper end resting upon the ring, and moves freely in it: on to the lower end of the wire G, is screwed one of the screw forceps, which receives one end of a wire, that is to be tried what force, or charge, it will bear before it will be melted, or destroyed.

Fig. 2, is a lump of lead, which is put anywhere upon the metallic covering on which the bottles of the battery stand: a hole is made in the lump of lead to receive one end of a wire, to the other end of which, is screwed the other screw forceps, which holds the other end of a wire, as the former.

Fig.

In Mr. Adams's first edition of his very useful *Essay on Electricity*, in the contents of the book, Chap. XVI. he says, *Mr. Brooke's Electrometer described*, but I cannot perceive how any person who had not seen my electrometer, could make out any thing of what mine is, either from what is given in that work plate 5, fig. 96, and 97, or by what is there said concerning it. That which is represented by fig. 96, is of the Gentleman's invention, mentioned in the preceding notes, which, he was so obliging as to shew to me, when he had nearly finished it, and which he afterward told me he had presented to Mr. Adams: who might be under an injunction not to add the inventor's name to it. The arms on that represented at fig. 97, have a resemblance to two of the
arms

Fig. 3, is a piece of glass fixed into a wooden handle, to press back and disengage the spring-catch before-mentioned.

arms on mine, but the method there given to graduate the uppermost arm, is very different from the method that I used to graduate mine; nor is that method, in my opinion, so accurate; for unless, in that method, the weights that are to be laid upon the ball be put exactly in a line over the points of suspension, the graduations must be very irregular. In the method I used, no such irregularity can arise, if any tolerable care be taken about it. In the third edition of Mr. Adams's Book, this article is wholly left out in the table of contents, and only noticed in the index to the work; where it is said, under the article ELECTROMETER, *new one similar to Mr. Brooke's*: but the similarity, there specified, seems to remain the same as it was in the former editions, both in the plates to the work, and in what is there said about it.

THE HISTORY OF THE UNITED STATES

CHAPTER I
THE DISCOVERY OF AMERICA
The first discovery of America was made by Christopher Columbus in 1492. He sailed from Spain and reached the island of San Salvador in the West Indies.

After his discovery, Columbus made several more voyages to the Americas. He explored the coast of Central America and the Gulf of Mexico. He also discovered the island of Cuba and the mainland of North America.

Columbus's voyages opened the way for European exploration and settlement of the Americas. His discoveries led to the establishment of colonies and the eventual independence of many of these countries.

The history of the United States is a story of exploration, discovery, and the struggle for freedom. It is a story that has shaped the world and continues to shape the future.

1776

THE DECLARATION OF INDEPENDENCE
The Declaration of Independence was adopted on July 4, 1776. It declared the United States to be a free and independent nation, no longer under British rule.

The Declaration was signed by the members of the Continental Congress. It was a bold statement of the American people's desire for self-government and freedom.

The Declaration of Independence is one of the most important documents in American history. It is a symbol of the American dream and the values of freedom, justice, and equality.

CHAPTER II.

MISCELLANEOUS EXPERIMENTS

O N

ELECTRICITY.

EXPERIMENT I.

WITH a battery of nine bottles, containing about sixteen square feet of coated surface, charged to thirty-two grains of repulsion, which charge was sent through a piece of *steel* wire twelve inches long, one hundredth of an inch thick, eleven times, when it was shortened one inch and a half, being then about ten inches and a half long; the twelfth time, the wire was melted to pieces.

E 2

EXPERIMENT

EXPERIMENT II.

A charge, with the same nine bottles, to thirty-two grains of repulsion, being sent through a piece of *steel* wire twelve inches long and one hundred and seventieth of an inch thick, the first time melted the whole of it into small globules.

EXPERIMENT III.

A charge of the same nine bottles charged to thirty-two grains, being sent through a piece of *brass* wire twelve inches long, one hundred and seventieth of an inch thick, the whole of it was melted, with much smoke, almost like gunpowder, but the metallic part of it after it was melted, formed itself, in cooling, chiefly into concave hemispherical figures of various sizes.

EXPERIMENT IV.

With only eight of the above bottles charged to thirty-two grains, the charge did but just melt twelve inches of the *steel* wire one hundred

dred and seventieth of an inch thick, . so as to fall into several pieces, which pieces in cooling, formed themselves into oblong lumps joining to each other by a very small part of the wire between each lump, which was not melted enough to separate, but appeared like oblong beads on a thread at different distances.

EXPERIMENT V.

The same eight bottles charged to thirty-two grains, so perfectly heated twelve inches of *brass* wire, about one hundred and seventieth of an inch thick, as to melt it, or soften it enough for it to fall down by its own weight (from the forceps with which it was held at each end) upon a sheet of paper placed under, to catch it; and when it fell down, it was so perfectly flexible, that by falling, it formed itself into a bent, or rather, vermicular shape, and remained entire its whole length, i. e. about twelve inches when it was put into the forceps; but after it was fallen on the paper, it sagged so much as to be

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be stretched by its own weight from twelve to about fifteen inches long; and by falling on the paper it flattened itself the whole length of it, that when it was examined with a half inch magnifier, it appeared about five or six times broader than it was in thickness.

EXPERIMENT VI.

With nine bottles again, charged only to twenty grains, the charge was sent through twelve inches of *steel* wire, one hundred and seventieth of an inch thick, which heated it enough to melt it, so as to be separated in many places; and the pieces formed themselves into string-bead-like shapes as in Experiment 4.

EXPERIMENT VII.

With the same nine bottles charged to twenty grains, the charge was sent through ten inches of *brass* wire one hundred and seventieth of an inch thick, the wire was heated so red hot, as to be very flexible, yet, it did not separate, but was shortened near three eighths of an inch.

REMARK I.

In the seventh experiment, the charge was not strong enough to heat the ten inches of *brass* wire, so as to be sufficiently softened, that it might stretch as in Exp. 5, but the flexibility was very plain, by its being made nearly straight by the stroke.

EXPERIMENT VIII.

A charge of nine bottles, charged to twenty grains sent a second time through the last piece of wire, melted it asunder in three places.

REMARK II.

The nine bottles charged to only twenty grains, in Exp. 6, 7, 8, seem to be of nearly the same power as eight of them charged to thirty grains, as in Exp. 4 and 5, but not exactly so, in Exp. 7, where the wire was *brass*, as in Exp. 5, the quantity of coated surface was one ninth more than the fifth Experiment, though the charge was not so high
by

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by one third, which is a great disproportion; but the length of the wire was less by one-sixth, which brings it much nearer; as will be explained afterward.

EXPERIMENT IX.

Nine bottles charged to thirty grains and the charge sent through twelve inches of *brass* wire, one hundred and seventieth of an inch thick, treated it nearly as in Exp. 5, except that it was separated in two places, and the pieces measured about sixteen inches and a half long; but perfectly flattened by its fall on the paper, as before.

EXPERIMENT X.

Nine bottles charged to thirty grains, and the charge being sent through eight inches and a half of *brass* wire the size of the last, wholly dispersed it in smoke, and left nothing remaining to fall on the sheet of paper, placed under it.

REMARK

REMARK III.

Experiments the ninth and tenth seem to shew, that something very considerable is superinduced or avoided, by the wire being longer, or shorter, with respect to its destruction, or preservation, see Remark 2.

EXPERIMENT XI.

With twelve bottles, charged to twenty grains, the charge was sent through ten inches of *steel* wire, one hundredth of an inch thick, which made the wire red hot, but did not melt it.

EXPERIMENT XII.

A second charge, the same as the last, was sent through the same piece of wire which heated it red hot as the first did, but it was not separated; this piece of wire was now shortened five sixteenths of an inch.

EXPERIMENT XIII.

A charge, to twenty-five grains, with the same twelve bottles, was sent through the
F last

last piece of wire, which melted it into many pieces, and many globules of calcined metal.

EXPERIMENT XIV.

A charge of fifteen bottles, charged to twenty-five grains, was sent through ten inches of *steel* wire one hundredth of an inch thick, which melted it the first time, and dispersed a great part of it about the room.

EXPERIMENT XV.

A charge with the last fifteen bottles, charged to twenty grains, just melted ten inches of *steel* wire, the size of the former, so as to run into beautiful globules, nearly as in Exp. 13.

REMARK IV.

This last Experiment so nearly resembles the 13th, that, exclusively of the wire having received two strokes before they perfectly agree. In this last experiment, the quantity of coated surface was one fifth part more than in

in the former, but the height of the charge was one fifth part less: in the former, the height of the charge was one fifth part more, but the quantity of coated surface was one fifth part less than the latter; so that the deficiency on one side, in either case, is made up by the excess on the other.

EXPERIMENT XVI.

A charge of fifteen bottles, charged to fifteen grains, being sent through ten inches of *steel* wire, the size of the last, it was barely made red hot; but it was shortened one-tenth of an inch, by the stroke passing through it.

EXPERIMENT XVII.

The last piece of wire having a charge of fifteen bottles, charged to twelve and a half grains, sent through it, did not make it red hot.

EXPERIMENT XVIII.

A charge of the same fifteen bottles, charged to twenty-five grains was sent through the same piece of wire, which, seemingly, tore the wire into splinters.

REMARK V.

This wire did not run into globules when it was separated, as in Exp. 15. The five grains, in this last, above the former, seem to have made the charge so strong, as to act upon the wire instantaneously: whereas, in most of the experiments, where the wire was melted into globules, the heat does not arrive at its height so rapidly; but its progress is often so slow as to make it very pleasing when the fusion comes on.

EXPERIMENT XIX.

Four bottles, charged to thirty grains, just melted three inches of *steel* wire, one hundred and seventieth of an inch thick, so as to fall into pieces.

EXPERIMENT XX.

Five bottles, charged to twenty-five grains, most beautifully melted three inches of such wire as the last, into large globules.

REMARK

REMARK VI.

The four bottles did not do quite so much as the five, in the two last experiments, although they were charged higher: the charge of the former was higher by one sixth part than the charge of the latter, but the quantity of coated surface in the five was one fifth more than in the four, so that the increase of surface in the five was something more in proportion than the increase of charge in the four. It is very probable that if the four bottles had been charged a single grain higher, their effect might have been equal to that of the five.

EXPERIMENT XXI.

Eight bottles, charged to fifteen grains, melted three inches of *steel* wire one hundred and seventieth of an inch thick, similar to the five in the last experiment; so nearly alike both in appearance and effect, that it might have been said to be the same.

EXPERIMENT

EXPERIMENT XXII.

Ten bottles charged to twelve and a half grains, rather exceeded Exp. 19, but scarcely came up to Exp. 20, and 21.

EXPERIMENT XXIII.

Suspecting something in Exp. 19, I found, that though my bottles hitherto, were as nearly of the same size as I could procure them, yet, some of them were a little larger than others, and, which was the case in Exp. 19, one of the four was smaller than the other three, so that I repeated the experiment with four bottles more equal in size, and charged them to thirty grains, and the fusion was as perfect as in any.

REMARK VII.

In Exp. 22, there were two small bottles amongst them: one of these small ones, was rather larger than the other; had I taken care to adjust the increase and decrease of charge, exactly in proportion to the augmentation and
diminution

diminution of surface, the last five experiments probably would have been, both in appearance and effect, so much alike as scarcely to have been distinguishable.

EXPERIMENT XXIV.

A charge to thirty grains with the same four bottles as in Exp. 19, was sent through a piece of *steel* wire six inches long and one hundred and seventieth of an inch thick; but it would not melt the wire this length; it was but barely made red, or rather of a purplish red.

EXPERIMENT XXV.

A charge to twenty-five grains with the same five bottles as in Exp. 20, affected six inches of wire like the last, very nearly in the same manner, as in the preceding experiment.

EXPERIMENT XXVI.

A charge to fifteen grains with the same eight bottles as in Exp. 21, only heated six inches of wire like the last, just as in Exp. 24.

REMARK

REMARK VIII.

The power of the charge in either of the three last experiments seems to be almost alike, and the only apparent cause why the wire was not melted as in Exp. 19, 20, 21, was the difference in the length of the wires, see Remark 2 and 3.

EXPERIMENT XXVII.

A charge to twenty grains, with the last eight bottles, very finely melted six inches of *steel* wire one hundred and seventieth of an inch thick.

EXPERIMENT XXVIII.

With two bottles, charged to forty-five grains, the charge was sent through one inch of such sized *steel* wire as the last, which only changed its colour.

REMARK IX.

The changing of the colour in the last experiment seems to shew that the charge passed

passed chiefly through the wire, as the spark, or light of the charge was not visible at the wire, although the ends of the forceps were so near to each other.

EXPERIMENT XXIX.

Three bottles, with a forty grains charge dispersed one inch and a half of *steel* wire, the size of the last, all about the room.

EXPERIMENT XXX.

As a *steel* wire of one hundredth of an inch thick has nearly double the quantity of metal of a wire one hundred and seventieth of an inch thick, so I took three inches of the former, and sent a twenty-five grains charge with ten bottles through it, which melted it, just as the five bottles did in Exp. 20.

EXPERIMENT XXXI.

Twenty bottles charged to twelve grains and a half, melted three inches of *steel* wire, the size of the last, exactly similar to the foregoing experiment.

G

EXPERIMENT

EXPERIMENT XXXII.

As a *steel* wire of one eightieth of an inch thick contains nearly twice the quantity of metal in the same length as a *steel* wire of one hundredth, or four times the quantity of a *steel* wire of one hundred and seventieth of an inch thick, so it might, from the foregoing experiments, be expected that twenty bottles, charged to twenty-five grains, would melt three inches of *steel* wire one eightieth of an inch thick, but on a great many trials twenty bottles could not be procured that would bear the discharge, when charged to twenty-five grains; for at the discharge there would be always one, or more bottle or bottles broken, or perforated. I was now reduced to the necessity of being content with getting bottles of any size that would bear the required charge, from one to three gallons each, or that contained from about one hundred and fifty, to three hundred, or more, square inches, of coated surface, each; but all in vain, my only resource left (as I was not near any glass-house) was, to increase the quantity of surface, and not to charge so high, and

and to proportion the one to the other, [see Remark 6,] a third part was concluded on to be tried, that is, instead of about thirty-six feet of coating, I added one third, or, twelve feet, which made it forty-eight feet: and that, instead of charging to twenty-five grains (or twenty-four grains) which divides by three better, to omit one third of the height of the charge, which leaves sixteen grains, and thus I succeeded perfectly well; for three inches of *steel* wire one eightieth of an inch thick was as curiously melted with forty-eight feet of coated surface, charged to sixteen grains, as any of the former*.

REMARK X.

These bottles, thus broken in large discharges, seem always to break, or to be struck

* The destruction of so many bottles in the last experiment so much altered the battery, which hitherto was not so commodious as could be wished, that I resolved to make the arrangement anew, and in the order, mentioned in the introduction to the work, by which any single bottle or any number of them may be disengaged very readily without removing any of the other, and in this form it has been continued.

G 2

through,

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through, nearly in the thinnest, but never in the thickest place, which shews the necessity of substance in the glass. [See introduction to the work.]

EXPERIMENT XXXIII.

As in Exp. 19, and 21, where the former is but half the quantity of coated surface of the latter, charged to thirty, and the latter to fifteen grains, to know how high forty-eight feet of coating must be charged to produce the same effect exactly: and as the quantity of coating in four bottles, consisting of a little more than six feet and a half, is contained in forty-eight feet a little more than seven times; so I tried by charging forty-eight feet only to a little more than four grains, or only about one seventh part so high, as four times seven is twenty-eight, that is, but two less than thirty, and this had exactly the same effect on the wire, which was one hundred and seventieth of an inch thick, and three inches long, as the former.

EXPERIMENT

EXPERIMENT XXXIV.

As the last experiment agreed so exactly with Exp. 19 and 20, the next thing tried was, to see the effect of forty-eight feet of coated surface charged to a little more than four grains, upon six inches of *steel* wire, the size of the last; but this was only made very faintly red.

EXPERIMENT XXXV.

A repetition of the last experiment with the same length of the same wire, to see how often the same charge might be sent through before it would be melted, and to observe the appearance of the wire after each stroke. The eighth stroke melted it into several pieces. After the first stroke the redness grew less every time, even the last time, when it was separated. The first stroke though little more than fairly red, made it so flexible, that by a little more than its own weight, (about a penny-weight more) it was apparently made perfectly straight when it was cooled: about the

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the third or fourth stroke it began to appear zigzagged; after the sixth stroke the surface of it appeared rough; after the seventh stroke the surface was very roughly scorified, or scaly; and some of the scales had fallen off upon a piece of white paper, placed under it, at about half an inch distance below it. The eighth stroke melted it in three places, and at those places where the angles appeared the sharpest, or most acute; a great number of the scaly appearances were driven off about the paper, which appeared like splinters, [see Exp. 18.] some of them were almost one tenth of an inch long, and some of them about a third or a fourth part of the diameter of the wire in breadth, and very thin: after the seventh stroke it was shortened seven sixteenths of an inch: the wire was one hundred and seventieth of an inch thick.

EXPERIMENT XXXVI.

Repeating Exp. 34, again, with the same size, and length of wire, and the same battery charged the same, in order to observe the method

thod of the wire shortening, having fixed an insulated gage parallel to, and about a quarter of an inch distant from it: after the first stroke, which made the wire fairly red, (it being fixed at one end, that the shortening might appear all at the other, which was held so as either to contract or dilate) I observed that it shortened considerably as it cooled; repeating the stroke, it did the same, and so on till it was melted, which was by the eighth stroke, as before. At the instant that the stroke passed through the wire it appeared to dilate a little, and after it was at its hottest, it gradually contracted after every stroke as it cooled, about one sixteenth of an inch each time, the dilating was so very little, as to bear but a very small proportion to its contraction, and sometimes it was doubtful whether, or not, it did dilate at all; but after all the observations it appeared oftener as if it did dilate, than as if it did not.

EXPERIMENT

EXPERIMENT XXXVII.

The same battery with forty-eight feet of coated surface charged to six grains, melted six inches of *steel* wire one hundred and seventieth of an inch thick, the most pleasingly into the finest string-bead shapes that I have hitherto observed.

EXPERIMENT XXXVIII.

The same forty-eight feet battery, charged to seven and a half grains, very finely melted twelve inches of *steel* wire the size of the last.

EXPERIMENT XXXIX.

The same forty-eight feet again, charged to six grains, would not make twelve inches of wire like the last, red hot.

EXPERIMENT XL.

The same forty-eight feet *negatively* charged to a little more than four grains, melted three inches of *steel* wire, one hundred and seventieth

tieth of an inch thick the same as the *positive* charge did in Exp. 33.

EXPERIMENT XLI.

The same battery of forty-eight feet of coated surface, charged to a little more than eight grains, melted three inches of *steel* wire, one hundredth of an inch thick. This is very nearly in proportion to Exp. 30, but here the charge was *negative*, and the fusion was the most pleasing of any I have hitherto had: probably owing to the charge, by chance, happening to be so well adjusted as to be exactly sufficient to melt the wire and no more: it held hot the longest, and the fused metal ran into the largest globules: probably the length of the time that the heat continued, was owing to the charge being just sufficient, and to the size of the lumps that the fused metal formed itself into.

REMARK XI.

The ten bottles in Exp. 30, were of such a size that the contents of their coated sur-

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face

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face was very near one third part of forty-eight feet, equal to the contents of coated surface in the battery used in the last experiment, that is, about eighteen feet, but the charge of the former being two thirds higher, made their effect so very nearly equal.

EXPERIMENT XLII.

Forty-eight feet as in last experiment, charged to nine grains, melted eighteen inches of *steel* wire one hundred and seventieth of an inch thick; it was melted at both ends into globules, and strung-bead shapes about four, or five inches at each end, but there remained about nine inches unseparated in the middle part of the wire, the ends of it were clearly made much hotter than the middle, which was made only fairly red, and cooled very soon; nevertheless, it was so heated, as to calcine, or scorify, the outside of the wire the whole length that remained unseparated; and when it was taken up, by handling, it bent angularly, and at every bending, the calcined, or scorified, metal broke away and
fell

fell off, leaving the internal part of the wire quite perfect, except that it was much diminished by the scorification; but whilst in the hands, it appeared, between the bendings, to be enlarged, about one third part in thickness.

EXPERIMENT XLIII.

A repetition of Exp. 1, with twelve inches of *steel* wire, one hundredth of an inch thick, but with this difference, that, as then I used only nine bottles, containing about sixteen square feet of coated surface charged to thirty-two grains, I here used eighteen bottles containing about thirty-two square feet of coating charge dto only sixteen grains. This was done, to observe the progress of the destruction of the wire, as in Exp. 35, as well as to prove the similarity of the effect. The wire being the same size, sort of metal, and length, as recited just above; the first stroke made it fairly red hot the whole length of it with smoke and smell, changed its colour to a kind of copperish hue, and shortened it considerably; the second stroke made it of a fine

H 2
blue,

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blue, but it did not appear red, and shortened it more; at the third stroke, it became zig-zagged, many radii were very visible at the bendings, and continued to shorten till the eleventh stroke, when one of the bottles in the second row of the battery was struck through, the fracture was covered over with common cement*, its place supplied by changing place with one in the third row, supposing the mended one to be the weakest, [see the introduction to the work] and thus, with the battery in this state, I made the twelfth stroke, which separated the wire, as in Exp. 1, but this wire was shortened only one inch.

* This cement is very cheap and easily made, as follows: take eight ounces of spanish white, heat it very hot in an iron ladle, to evaporate all moisture, and when cool, sift it through a lawn sieve, then add three ounces of pitch, three quarters of an ounce of rosin, half an ounce of bees-wax, heat them altogether over a gentle fire, till the mixture is pretty hot, frequently stirring it, for near an hour; then take it off the fire, continue the stirring till it grows cool, and it is fit for use.

REMARK

REMARK XII.

The ceasing of the appearance of the wire being heated red hot after the first stroke, seems to be owing to the first stroke, in its passage, calcining the outside of the wire to a certain depth or thickness, which calcination remaining upon the wire, conceals the redness at the second and succeeding strokes. In Exp. 42, is a sufficient proof of the calcining the outer surface of the wire when it only appears fairly red hot: in that experiment is a proof of various degrees of heat, produced at the same time, from perfect destruction of the wire to its appearing only fairly red hot. What advantage such experiments as this may be of, in determining the direction of the electricity, time and perseverance may shew.

EXPERIMENT XLIV.

A repetition of Exp. 43, with the same eighteen bottles, charged the same, and a piece of wire the same length and size; relative to the wire being heated red hot the first time
 and

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and not after, till the twelfth stroke, when it was melted. The first stroke made it red hot, and produced much smoke, as before, but no smoke or redness at the second stroke; after the first stroke it was of a copper colour, but of a fine blue after the second, and began to be zigzagged, it was likewise shortened one tenth of an inch, by these two strokes.

EXPERIMENT XLV.

With the last eighteen bottles charged to twelve grains, the charge was sent four times through ten inches of *brass* wire, one hundred and sixtieth of an inch thick; the fifth stroke separated it, and shortened it thirteen sixteenths of an inch, it was also made red hot by every stroke.

EXPERIMENT XLVI.

A twelve and a half grains charge with eighteen bottles, sent through three inches of *steel* wire, one hundred and twentieth of an inch thick very finely melted it.

EXPERIMENT

EXPERIMENT XLVII.

A twelve and a half grains charge with forty-eight feet of coated surface was made to pass through three inches of *brass* wire, one hundred and twentieth of an inch thick, which just heated it red hot enough to fall into several pieces.

REMARK XIII.

From the two last experiments it appears, that a *brass* wire, of the same length and size of a *steel* one, will bear a charge one third part stronger.

EXPERIMENT XLVIII.

A charge of eighteen bottles, or about thirty-two square feet of coated surface, charged to ten grains, sent through three inches of *steel* wire, one hundred and twentieth of an inch thick, heated it fairly red hot.

EXPERIMENT XLIX.

The same eighteen bottles charged to eleven grains, the charge being sent through the
 REMARK forme

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former piece of wire, melted it into several pieces; but those parts that remained in the form of wire were very much shivered as in Exp. 18, and 35.

EXPERIMENT L.

The last eighteen bottles charged to eleven grains, and the charge sent through three inches of *steel* wire the size of the last, heated it very red hot, but did not separate it.

EXPERIMENT LI.

A charge of forty-eight feet, to eleven grains, heated three inches of *brass* wire, one hundred and twentieth of an inch thick red hot, but would not separate it, for it remained whole in the forceps after the stroke had passed through; but it was so very near being separated by the stroke, that it parted on letting it out of the forceps: the stroke shortened the wire one twentieth of an inch.

REMARK

REMARK XIV.

The two last experiments prove what is advanced in Remark 12, but the *brass* wire does not appear to scorify with the stroke so much as *iron* or *steel*.

EXPERIMENT LII.

A charge of forty-eight feet to eight grains sent through three inches of *copper* wire, one hundred and seventieth of an inch thick, seven times, made it zigzagged, but not much shorter, the eighth stroke separated it at one end, close to the forceps which held it, but it did not appear to be made sensibly red hot at all, notwithstanding it must have been often so at the place where it was melted: which space was so very small as barely to be perceptible, like as when a point is set upon any flat surface of *iron*, and a stroke from a pound phial being sent through, both the point and the flat surface where the point rested, if examined with a magnifying glass, will be found to have been melted, and a speck may be seen;

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but

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but the redness of the metal will scarcely be visible.

REMARK XV.

As observed in the last experiment with a point and a pound phial, I have often melted holes in a thin sheet of *brass*, such as those from which grain weights are cut.

EXPERIMENT LIII.

A repetition of the last experiment, only adding two grains to the charge, that is, ten grains, instead of eight, as in the last, with a piece of *copper* wire the same length and size, which was, by the charge, dispersed all about the room.

EXPERIMENT LIV.

A charge of forty-eight feet to twelve grains, sent through three inches of *copper* wire, one hundred and fortieth of an inch thick, heated it red hot, enough to separate it in several places.

EXPERIMENT

EXPERIMENT LV.

A repetition of the last experiment, with the same success, every circumstance being alike both in the wire and the charge.

EXPERIMENT LVI.

A charge of forty-eight feet to six grains sent through three inches of *steel* wire, one hundred and seventieth of an inch thick, dispersed the whole of it in red hot globules about the room.

REMARK XVI.

The additional resistance that is made, to the electrical stroke, by the additional length of a wire, as observed in Remark 3, very plainly appears in this last experiment; and the observation seems to hold good either with large or small quantities of coated surface.

EXPERIMENT LVII.

A charge of forty eight-feet to five grains, sent through three inches of *steel* wire, one
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hundred

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hundred and fortieth of an inch thick, heated it red hot so that the surface of the wire was scorified its whole length; it did not melt it into pieces, but shortened it one twentieth of an inch.

EXPERIMENT LVIII.

A charge of forty-eight feet to six grains, and sent through three inches of *steel* wire one hundred and fortieth of an inch thick, melted it into fine globules.

REMARK XVII.

Hence it appears that a piece of *copper* wire, the same size and length, will bear twice the charge that a *steel* wire will, [see Experiment 54.]

EXPERIMENT LIX.

A repetition of the last experiment having every circumstance alike, and the effect was the same: this repetition was only to ascertain the fact more fully.

EXPERIMENT

EXPERIMENT LX.

A charge of forty-eight feet of coated surface to between ten and eleven grains was sent through three inches of *steel* wire, one hundredth of an inch thick, which just melted it so as to separate.

REMARK XVIII.

Hence it seems as if a *copper* wire of one hundred and fortieth of an inch thick, would bear as great a charge as a *steel* wire of double the size and equal in length; or that the *copper* wire would be rather the safest.

EXPERIMENT LXI.

A charge of forty-eight feet, charged to twelve grains, being sent through one inch and a half of *steel* wire, one hundredth of an inch thick, the wire was dispersed all about the room.

EXPERIMENT LXII.

The same charge as the last was sent through one inch and three eighths of *copper* wire one hundred

hundred and fortieth of an inch thick, but the charge did not separate it.

R E M A R K X I X .

Now it seems to be confirmed, that a *copper* wire of almost any length and size, will be equally safe as a *steel* wire of the same length and double the size. And this wire not being melted as in Exp. 54, the charge and surface being equal, seems to be owing to the forceps being too near, for the whole of the charge to pass solely by the wire, as in Exp. 28.

Hitherto it may appear as if more attention had been given to the different sizes of wires (and the sorts of metal) than to the length of them; but the knowledge of the latter, is as satisfactorily obtained, as the former, from the same experiments; for it appears from Experiments the 2d, 19th, and 21st, that a certain quantity of coated surface charged to a certain height, will melt a wire of a certain size and length of a certain metal. It appears likewise, from the same experiments, that
double

double the quantity of coated surface, charged to the same height, will destroy four times the length of the same wire; as the length of the former, was four times that of the latter, and the quantity of coated surface in the former, was about double that of the latter; but the charge was nearly equal in both. Or the observation may be changed, thus, that, as already observed, a certain quantity of surface, charged to a certain height, will melt a wire of a certain size and length of a certain metal; so it appears from various other experiments, that the same quantity of surface, charged twice as high, will do the same execution, as twice the quantity of surface charged half as high.

REMARK XX.

At Experiments 33 and 34, let the mean of the length of the wires, in the two experiments, be taken; and it will agree, as near as possible, with Exp. 42, in the same proportion as the foregoing. In Exp. 33, the wire was three inches long, but it was all melted

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melted. In Exp. 34, the wire was six inches long, but none of it was melted. Now the mean length of the wires in the above two experiments is four inches and a half; which is just equal to one fourth part of the length of the wire in Exp. 42: and combining those three experiments, the effect is produced as in experiments the 2d and 21st. And whenever these experiments shall be repeated, with the like apparatus, I am inclined to think that the result will be found very nearly the same.

EXPERIMENT LXIII.

A charge of forty-eight feet, to five grains, sent through three inches of *lead* wire, one sixty-sixth of an inch thick, melted it into many different pieces, and lumps of lead.

EXPERIMENT LXIV.

A charge of forty-eight feet to four grains, melted three inches of *lead* wire, one sixty-sixth of an inch thick, asunder, in three different places.

EXPERIMENT

EXPERIMENT LXV.

A charge like the last was sent through six inches of *lead* wire like the last, which only melted it asunder about the middle of it.

EXPERIMENT LXVI.

A charge of forty-eight feet to eight grains was sent through three inches of *lead* wire one fifty-sixth of an inch thick, which melted it into many pieces.

EXPERIMENT LXVII.

A charge like the last was sent through six inches of *lead* wire the size of the last, which only melted it asunder in one place near the middle.

REMARK XXI.

As a *steel* wire of one fifty-sixth of an inch thick appears to contain nearly four times as much metal in the same length as a wire of one hundredth of an inch thick; and as it took a charge of about eight grains with forty-

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eight

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eight feet of coated surface, to destroy three inches of the latter; [Exp. 41] so it appears that a *steel* wire would be four times as effectual against the electricity or lightning, as a *lead* wire of the same length and size.

EXPERIMENT LXVIII.

A charge of ten grains, from forty-eight feet, sent through three inches of *lead* wire, one fiftieth of an inch thick, made no sensible alteration in it.

EXPERIMENT LXIX.

A charge of forty-eight feet, to twelve grains, sent through three inches of such *lead* wire as the last, melted it into several pieces.

EXPERIMENT LXX.

A charge like the last was sent through six inches of such wire as the last, which only melted it asunder in one place.

EXPERIMENT

EXPERIMENT LXXI.

A charge of forty-eight feet, to sixteen grains, was sent through six inches of *lead* wire, one fiftieth of an inch thick, which melted it into many pieces.

EXPERIMENT LXXII.

A charge of forty-eight feet, to fifteen grains, was sent through six inches of wire like the last, which did not separate it, but made it smoke.

EXPERIMENT LXXIII.

A charge like the last was sent through the last piece of wire a second time; which melted it into several pieces.

REMARK XXII.

The law by which wires resist destruction, in proportion to the thickness of the wire, does not seem to be so equable, by much, in the *lead*, as in the *steel* wire. For a charge of four grains, in Exp. 64, melted three inches of *lead* wire, one

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sixty-sixth of an inch thick: but it took a charge of about three times that power to destroy three inches of *lead* wire one fiftieth of an inch thick; which is about double the quantity of metal in the same length as in that of one sixty-sixth of an inch thick. [see Exp. 69.] Thus it is easy to find, what different resistance a wire of any of the foregoing metals, of equal size and length, will make to the electrical stroke, or to lightning.

The length of the electric circuit, in which the different wires were placed, in the foregoing experiments, from the nearest part of the inside, to the nearest part of the outside, of the battery, exclusive of the length of the said wires, was about eight feet.

Notwithstanding the easy destruction of the *lead* wire by the electrical stroke; it seems greatly to be doubted, whether any thunder strokes happen in any place whatever, strong enough to destroy a strip of lead four inches broad and of the thickness of about eight pounds

to

to the foot. Whence it may be presumed, that such a strip of lead may be perfectly safe for conductors through buildings of any kind whatever: as it is not much subject to decay in any common exposure.

The most remarkable instance of the effect of a thunder stroke, which ever happened in the limits of my knowledge, was at a stable belonging to the Rev. Dr. Sanby, at Denton, about fourteen miles from Norwich; which happened on the 28th of July 1775, at about six o'clock in the afternoon: where the gable end of the stable, which faced nearly west, was very much damaged; and on the top of the gable, which was brick work, was fixed a vane on an iron rod, whose length was about eight feet; and, for steadiness, it was wrought up in the brick-work near three feet down. In the roof of the stable is a hay-loft; and about two feet below the floor of the hay-loft, about the middle, within the stable, into the brick-work, was driven a large iron spike; and upon this spike was hung a pair of iron
yangles,

*yangles**, as they are called. The stroke of the lightning seems to have taken its passage by this pair of *yangles* as they hung, and the rod of the vane; and in its passage the *yangles* were so compleatly fixed, at the joinings of the links, by being melted with the stroke, that the whole of the chain remained perfectly stiff and straight, and adhered so firmly together, that the *yangles* were carried to the blacksmith's to be set loose again, and made fit for use. The rod of the vane, and the vane itself, were wholly misplaced, and never yet were found any where, nor is it expected now, will ever be found. Great part of the bricks of the gable were thrown in various directions; some of them were thrown over part of the dwelling-house, which was considerably higher than the stable, to the distance of between eighty and ninety yards; [which I measured by pacing the distance:] many of the windows of the dwelling-house, which was at

* A kind of fetters made of strong iron links, to fix on a horses foot, to prevent them from running away or breaking pasture.

about

about twenty-four yards distance, were very much broken. A man servant, who was in the stable, was considerably benumbed, but recovered himself in a few hours after. There were, likewise, some horses in the stable, but they received no hurt.

Hitherto, the state of the clouds and the earth, in the time of thunder storms, appears to be very far from being well ascertained. But whoever has an insulated conductor continued into a dry and close room, may find, that changes in the air, or the rain, snow, or hail falling from the clouds, from negative to positive, and from positive to negative, happen so very often, that it does not appear easy to discover the state of the earth at such a time. Many of these changes may be observed to take place, by a pair of small cork balls, suspended on linen threads from the insulated conductor, in the space of ten minutes; particularly when the clouds and the thunder are near. When there is no thunder, or lightning, but only rain, snow, or hail, those changes will

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will often be not more than twice, thrice or four times during the whole time that the cloud is passing over, and sometimes only once.

A little after five o'clock in the morning of July 7, 1784, there happened a very violent storm of thunder and lightning at Bramerton, about four miles from Norwich, which did considerable damage at the dwelling-house of Mr. John Gurney; where the lightning seemed to take its course by a bell wire, part of which was brass, and the other part iron. That of iron was of the size called No. 21, and the brass No. 20, so that the brass wire was the thickest; and it was melted into many pieces; these I picked up in different rooms which it went through; but the bell-wire being continued into a third room, and the continuation being of iron wire, and smaller than the brass one, the whole of it was dispersed in smoke; a great quantity of which adhered to the wainscot of the room, and nothing else of the wire remained, except at the ends

ends where it was fastened to the bell-drags; but there, the wire was double, being twisted one to the other, and both the iron and brass remained unhurt. These I preserved in order to examine as to their size and weight. The brass wire, which was just separated, being the size of No. 20, I procured a piece of iron wire, the same size, and cut it three inches and a quarter long, which then weighed six grains and seventy-nine hundred and twenty-eighths of a grain, nearly six grains and five eighths.

By calculating from the foregoing observations, it appears that, for the iron wire to have been equal in power against the lightning to the brass one, the size of it must have been so much larger that three inches and a quarter in length must weigh nearly nine grains; which appears to be a little thicker than that which is called No. 19.

It has already been observed that, double the quantity of surface charged the same, will
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destroy

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destroy four times the length of wire. [see Remark 16.]

In Experiment 32, it plainly appears, that forty-eight feet of surface, charged to sixteen grains, melted three inches of *steel*, or *iron* wire one eightieth of an inch thick; from which, if the rule holds good, eight hundred and sixty-four square feet of coated surface, charged to sixteen grains, should melt three inches of *iron* wire, equal in power against the lightning to the brass one melted at Mr. Gurney's. And hence, presuming the rule to be good in all cases, which I hope will be tried, if Mr. Gurney's wires had all been iron, of the size of about No. 19, or equal in power to the brass one, in preserving itself, a charge of one thousand four hundred and forty feet of coated surface* charged to sixteen grains, having been made to pass through their whole length, which altogether was about forty feet, would have destroyed them in the same manner as the lightning.

* A surface of about thirty-eight feet square.

The

The following is an instance which I never happened of before, or ever repeated.

Two Gentlemen coming in, to see a piece of wire melted by electricity, I proceeded to shew it them, by fixing twelve inches of *steel* wire, one hundred and seventieth of an inch thick, in the forceps, and then (supposing the electrometer and all other things ready placed) to charge the battery, but the electrometer did not move; nevertheless I continued charging, as I supposed; but still the electrometer remained as it was, although I had been charging much longer than would have been necessary; contrary to my design, which was to take a small wire, that a small charge might be sufficient. Having been charging a long time, I left off to look about the apparatus, in order to see if any thing was not right: as I was looking, I found there was no communication to the electrometer, and heard a small crackling in the battery, which convinced me that it was charged. Accordingly I made the discharge, expecting nothing unusual; but the

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wire was dispersed seemingly in a very violent manner. The report was so very loud that our ears were stunned, and the flash of light so very great, that my sight was quite confused for a few seconds. The singularity of the appearances attending this experiment led me to insert it.

CHAPTER

CHAPTER III.

MISCELLANEOUS OBSERVATIONS

ON THE

LEYDEN PHIAL.

NOTWITHSTANDING the number of instructive as well as entertaining experiments on electricity, and in particular those, in which the Leyden Phial is used, most of those Gentlemen who have been pleased to consider me as one of their philosophical correspondents on this subject, seem always to have considered the outside and inside of the Leyden Phial, at the time that it is charging, to be in opposite states, that is, one *negative*, and the other *positive*; and all of them

them quote Doctor Franklin, in support of their opinion.

The late Mr. James Ferguson* was firm in this opinion, till I shewed him that this was not the case.

Another Gentleman to whom I had mentioned it in the year 1775, writes as follows, 'I am astonished at what you tell me, relating to a charged Leyden Phial, and long much to see your experiment. Till I do, I confess I cannot help thinking there must be some fallacy in it, which has escaped both yourself and Mr. Ferguson. If however the fact be true, that both sides of the phial are negative, or both sides positive, the whole of the present theory must fall to the ground †.'

* Mr. Ferguson gave a course of Lectures in Norwich in the year 1769, and the same again in April 1775.

† I had mentioned to this Gentleman that Mr. Ferguson had been with me; but instead of my experiment militating against the present, that is, Dr. Franklin's Theory, I think it very strongly confirms it.

Likewise

Likewise that very excellent philosopher and electrician, the late Mr. Wm. Bewly, seems to have been firmly established in the same opinion, who, in the year 1780, in a letter to the Rev. Mr. Morgan, speaking of two Leyden Phials, one hanging to the tail of the other suspended from the prime conductor, and describing them by calling the wire that enters the mouth of the uppermost, or first phial *P*, the wire that leads from the coating of the first, and enters the mouth of the second phial *p*, the coating of the first *N*, and the coating of the second *n*, with a chain leading from the last to the floor, writes as follows: ‘ *I wonder how Mr. Brook, with his well known experience in electricity, can consider N and p, as in the same state; to shew that they are not; it is sufficient only to remove them from each other: when he will find that p is positive, and N negative he may take a shock from them by bringing them together. The puzzle arises from the same conducting substance being connected with both.*’

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Is there not as much reason for any one to wonder, how any *electrician* can consider any two perfect metallic substances in actual contact with each other, to be in different states of electricity, except, as in Wilson and Hoadly's Experiments; and I never said that they were not so, when they were separated. Mr. Bewly adds, '*it may be said there ought to be no explosion between P and p, because they are both positive,*' this is certainly true, '*but p, has a metallic communication with N, the negative outside of the uppermost phial,*' here is a mistake; for this outside is not negative, at least the coating of it, but positive, having a complete communication with p, the positive inside coating of the lowermost phial. '*The same reasoning will apply to the explosion which takes place between N and n, n is undoubtedly negative:*' this is also certainly true: '*and so is N,*' but this is plainly a mistake again, for N is not negative, as already observed,

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In the preceeding section Mr. Bewly says, 'It may be said there ought to be no explosion between P and p , because they are both positive,' but their being both in a positive state does by no means shew that there ought to be no explosion, when bottles are thus situated; for it is generally allowed, that where there are unequal portions of the electric fluid, either negatively, or positively, if a proper communication be made between them, an explosion will take place: and this appears to be the case between P and p , at least as to their coating; for admitting that no more of the electric fluid can be driven into the inside of a bottle, than can be permitted to leave the outside, and that that which is driven from the outside of the first phial is driven into the inside of the second; and admitting that all the time the bottles are charging, each bottle will possess its natural ^{quantity} ~~quantity~~; yet on ceasing to charge them higher, or on ceasing to work the machine, the portion which was driven from the outside of the first phial into the inside of the second, will, by the metallic

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communication between them both, take place not only in the inside of the second phial, but will return and take place on the outside of the first, at least so far as their coatings are considered to be concerned, yet the inside of the first continues as it was when the machine ceased working. Thus the inside of the first phial will contain a quantity of the electric fluid nearly equal to the quantity occupying its own outside and the inside of the second; therefore, by the inside of the first being now so highly charged in proportion to its outside, if a communication be made with an insulated discharging rod from its wire to its coating, or from its coating to its wire, an explosion will succeed, yet neither of the bottles will be discharged; but instead thereof, the inside of the second phial and both sides of the first, whilst the communication is continued, will all be charged equally high and of the same quality, that is, positive: supposing the rubber of the machine not insulated. Thus it seems as if a coated glass phial may be made to contain more than its

its natural quantity; for it does not clearly appear, that the coating alone will contain all this additional quantity about the bottle.

Notwithstanding all that has been done with, or said about, the first phial, with respect to the great quantity that the inside of it contained when the machine ceased working; a few experiments will shew, that the same inside may be altered from being so strongly positive, to negative, or in the same state as the outside of the second phial at the time that it had a communication by a wire to the floor, without discharging the phial or any more working the machine, and will also, at the same time, afford many proofs of the truth of this seemingly new conceived opinion.

EXPERIMENT I.

For the sake of convenience let the apparatus be a little altered; and instead of two phials being suspended from the prime conductor, let two pound phials be coated with tin-foil on their outsides, and filled to a

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convenient

convenient height with common shot, to serve as a coating within-side, as well as to keep a wire steady in the phials without a stopple in the mouth of them. Let each phial be furnished with a wire, about the size of a goose quill, and about ten inches long, and let each wire be sharpened a little at one end, that it may the more easily be thrust down into the shot, so as not to touch the glass any where at the mouth of the phials, yet, so as to stand steadily in them. Let a metallic ball about 6 or 7 eighths of an inch diameter be screwed on at the other end of each wire: also let there be in readiness a third wire, fitted up like those for the phials, except that another ball of nearly the same size as the former, may occasionally be screwed on over the sharpened end of it. I say, instead of suspending the phials from the prime conductor, as before, let one of those above-described, be charged at the prime conductor, and then set it aside, but let it be in readiness in its charged state; then let the other be placed upon a good insulating stand, and let the third wire also be laid

laid upon the stand, so that its ball, or some part of the wire, may touch the coating of the phial. Let the sharpened end of this wire project five or six inches over the edge of the stand: all of these being now placed close to the edge of a table, hang a pair of cork balls on the sharpened end of the wire, and make a communication from the prime conductor to the ball on the wire in the bottle: on working the machine, the sharpened end of the wire will permit the bottle to be charged although it be insulated, and if the wire be very finely pointed, the bottle may be charged nearly as well as if it were not insulated: I say, on working the machine, the phial will charge, and the cork balls will immediately repel each other: but whilst this phial is charging take the first phial, which having been previously charged at the same prime conductor, in the hand, and while the second phial is charging, present the ball of the first to the cork balls, and they will all repel each other. This plainly proves that the outside of the second bottle is electrified plus, at the time that

that it is charging, the same as the inside of the first, and the inside of both the bottles will readily be allowed to be charged alike, that is, plus, or positive.

EXPERIMENT II.

Let the second bottle in the last experiment be wholly discharged, and charge it again as before, (the first bottle yet remaining charged) and whilst it is charging, let the ball of the first approach the cork balls contiguous with the second, and they will, as before, all repel each other: withdraw the ball of the first, and so long as the machine continues to charge the second bottle higher, the cork balls will continue to repel each other, but cease working the machine, and the cork balls will cease to repel each other till they touch, and will then very soon repel each other again; then let the ball in the first phial approach the cork balls, and they will now be attracted by it, instead of being repelled as above, as in the last experiment. This also plainly shews, that both sides of a Leyden Phial

Phial are alike at the time it is charging, and at the same time evidently shews, that the difference of the two sides does not take place till after the bottle is charged, or till the machine ceases to charge it higher.

EXPERIMENT III.

In this experiment, let both the former bottles be discharged, then let one of them be placed upon the insulating stand. Let a ball be put on over the sharpened end of the third wire, and let it be laid on the stand as before, so as to touch the coating of the phial: place the other phial on the table, so that its ball or wire may touch the ball on, or any part of, the third wire: make a communication from the ball on the wire of the first phial, to the prime conductor: then, by working the machine, both bottles will soon become charged. As soon as they are pretty well charged, and before the machine cease working, remove the second phial from the third wire; after the second phial is removed, cease working the machine as soon as possible:
take

take the third wire, with its two balls, off the stand with the hand, and lay it on the table, so that one of its balls may touch the outside coating of the second phial: remove the first phial off the stand, and place it on the table so as to touch the ball at the other end of the third wire; then, with an insulated discharging rod, make a communication from the ball in one bottle to the ball in the other: if the outside of the first phial be negative at the time it is charging, the inside of the second would be the same, and making the above communication would produce an explosion, and both bottles would be discharged, but the contrary will happen, for there will be no explosion, nor will either of the bottles be discharged, although there be a compleat communication between their outsides, because the inside of them both will be positive. This is a proof, that considering one side of a phial to be positive and the other negative, at the time they are charging, is a mistake: as well as that, if any number of bottles be suspended at the tail of each other all the intermediate

intermediate surfaces, or sides, do not continue so.

EXPERIMENT IV.

Here also let the apparatus be disposed as in the last experiment, till the bottles are highly charged: then, with a clean stick of glass, or the like, remove the communication between the ball of the first phial and the prime conductor before the machine ceases working; then, with an insulated discharging rod, make a communication from the outside to the inside of the first phial; a strong explosion will take place on account of the excess within-side, notwithstanding they are both positive: for as it is expressed in page 63, the inside of the phial possessed a much greater quantity than its outside, contrary to Mr. Bewly's observation, *'it may be said there ought to be no explosion between P and p, because they are both positive.'*

EXPERIMENT V.

This experiment being something of a continuation of the preceeding one; immediately after the last explosion takes place, discharge the prime conductor of its electricity and atmosphere; then touch the ball in the first phial with the hand, or any conducting substance that is not insulated; then will the inside coating of the first phial, which at first was so strongly positive, be in the same state as the outside coating of the second, having a communication, by the hand, the floor, &c. with each other; that is, negative, if any thing can properly be called negative, or positive, that has a communication with the common stock: but a pair of cork balls that are electrified either plus, or minus, will no more be attracted by either the inside coating of the first phial, or the outside coating of the second, then they will by the table on which they stand, or a common chair in the room, while they continue in that situation. Remove the aforesaid communication from the

the ball of the first phial; touch the ball in the second, as before in the first, or discharge the bottle with the discharging rod, and the ball in the first bottle will immediately become negative: with a pair of cork balls, electrified negatively, approach the ball in the first phial, and they will all repel each other, or, if the cork balls be electrified positively, they will be attracted. All these circumstances together seem fully to prove what has already been said, not only that the inside of the first phial, which was so strongly positive, may be altered so as to become in the same state as the outside of the second, without discharging the phial, or any more working the machine; but that it may be fairly changed, from being positively charged to being negatively charged. If a pair of cork balls are now hanged on to the ball on the wire in this phial, by the help of a stick of glass, they will repel each other, being negatively electrified. Make a communication from the outside of the bottle to the table, and replace the communication from the prime conductor

to the ball in the bottle, then, upon moderately working the machine to charge the bottle, the cork balls will cease to repel each other till they touch, and will soon repel each other again by being electrified positively. Here the working the machine anew, plainly shews that the inside of the first bottle, which was positive, was likewise changed to negative.

Although Dr. Franklin has been so often referred to in support of the opinion that the two sides of the Leyden Phial are in different states, at the time it is charging, it does not appear any where, in his most excellent book on the subject, that he takes any notice concerning them at that time; but almost invariably stays till it be charged, before he says any thing about its different states; from which there is some appearance, either that he has not been rightly attended to, or that he has been wrongly understood, although he writes almost in general with very great perspicuity. In that same Book [see Page 141.

Edit.

Edit. 1774] Mr. Colden has a very strong expression, that both its sides are alike at the time it is charging, in his remarks on the Abbé Nollet's Letters to B. Franklin, Esq. where the Abbé says *'that he can electrify a hundred men, standing on wax, if they hold hands, and if one of them touch one of these surfaces (the exterior) with the end of his finger,'* Mr. Colden then immediately says, *'This I know he can, while the phial is charging, but after the phial is charged I am as certain he cannot.'* And I cannot see how any one can draw such satisfactory conclusions from any part of Dr. Franklin's writings, as what those Gentlemen, already recited, as well as various others, seem to have done, on this subject, for the contrary appears plainly to be the fact.

In making electrical experiments, and in particular those in which the Leyden Phial is concerned (a number of which together compose most electrical batteries) a method to preserve the bottles, or jars, from being struck through by the electric charge, is very
desirable;

desirable; but I do not know that it has hitherto been accomplished. The number of them that have been destroyed in the foregoing, as well as in many experiments, made long before, have led me to various conjectures to preserve them: at the same time I have been obliged to make use of bottles instead of open mouthed jars. And as coating the former within-side is very troublesome, it has put me on thinking of some method more easy, quicker, and equally firm and good, as with the tin-foil. With respect to the new method of coating I failed: though something else presented itself rather in behalf of the former: therefore introducing the process here will not be of very great use; unless, in saving another the trouble of making use of the same method, or giving a hint towards the former, so as to succeed with certainty. My aim was, to find something that should be quick and clean, and not easy to come off with the rubbing of wires against it, and yet a good conductor. My first essay was with a cement of pitch, rosin, and wax, melted together; into which,

which, to make it a good conductor, I put a large proportion of finely sifted brass filings. When this mixture was cold, I put broken pieces of it into the bottle, and warmed the bottle till it was hot enough to melt the cement in it so as to run, and cover the bottle within-side, then I coated the outside with tin-foil as is commonly done, and now it was fit for use, or ready to be charged: to which I next proceeded; and I believe I had not made more than four or five turns of the winch before it spontaneously struck through the glass with a very small charge; I then took off the outside coating, and stopped the fracture with some of my common cement, after which I put the coating on again; and, in as little time as before, it was struck through again in a different place: and thus I did with this bottle five or six times; sometimes it struck through the cement; but it struck through the glass in four different places. This made me consider what it might be, that facilitated the spontaneous striking through the glass, and likewise what might retard it. I had long
before

before thought, that jars or bottles appeared to be struck through with a much less charge, just after their being coated, or before they were dry, than when they had been coated long enough for the moisture to be evaporated from the paste with which I mostly lay on the tin-foil; and could only consider the dry paste as a kind of mediator between the tin-foil and the glass, or in other words, that the moisture in the paste, was a better conductor, and more in actual contact with the glass, than the paste itself when dry. And the coating the bottles with the heated cement, though long afterward, did not alter my former idea; for it appeared as if the hot cement, with the conducting substance in it, might be still more in actual contact with the glass, than the moisture in the paste. On these probabilities I had to consider what might act as a kind of mediator more effectually than the dry paste, between the glass and the tin-foil. It occurred, that common writing-paper, as being neither a good conductor nor insulator, might be serviceable by being first pasted smoothly

to the tin-foil, and left to dry. The paper then being pasted on one side, having the tin-foil on the other, I put them on the glass together with the tin-foil outward, and rubbed them down smooth. This succeeded so well that I have never since had any struck through that were thus done, either common phials, or large bottles which contain near three gallons each, though some of the latter have stood in the battery in common use with the other a long time. And as I have never had one struck through that has been prepared in this way, I am much less able at present to tell how great a charge they will bear before they are struck through, or whether they will be struck through at all.

In the year 1769, as already observed, Mr. James Ferguson read a course of lectures in this city, at which I attended, and those were the first I ever saw. One of the lectures was on electricity; and his machine seemed to me to act considerably better than any one I had met with before; and, to in-

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crease the excitation, he put a piece of silk between the rubber and the cylinder, (but it was not made fast to any thing) and applied some amalgam, on a piece of sponge, to the cylinder. The advantage gained by it seemed to me to be very considerable; insomuch that I could not satisfy myself without applying the like to my own machine, which was soon done, but as it was not fastened to any thing, it was troublesome by sliding away. In order to prevent this, I made the silk fast to the rubber, by which I found I not only got rid of the trouble of frequently having to replace the silk, but I got more fire: this put me on fixing the silk in different positions, and in different shapes and sizes, and in a short time I got it to as great perfection as it has acquired ever since. At that time by its being entirely new, as to the quantity of electric fire it enabled an operator to produce, many people, from different parts of the kingdom, called on me to see it, by which means the use of the silk soon became pretty general, and has continued so ever

ever since, without any material alteration.

Although I had been conversant in electricity from the year 1749 to 1770, I never saw any machine that would produce the quantity of electric fire that my own, by the help of the silk, would then do; nor could I ever before charge jars, or phials near so high: and if a jar, or phial, at any time discharged of itself, or spontaneously, it was attributed to the excellency of the machine. But after I had so advantageously applied the silk, upon making electrical experiments very frequently, I found that the spontaneous discharging of a phial, &c. depended much more on another circumstance, that is, their being very clean and dry; and that if the phial or jar, had been warmed (which seldom used to be done) to make it still more dry and clean, the spontaneous discharging of it was very considerably facilitated: insomuch that it was very obvious, that a jar, or the like, would not take so great a charge when quite clean and dry as

it was otherwise capable of being made to take; the report of the discharge, as well as the time it took to be charged, were so very different, that the fact could not easily be doubted.

Although the difference of the charge appeared so very plainly, even to a by-stander, to be very great, yet I could not tell what that difference was, till I contrived my new electrometer; which enabled me to tell the difference very exactly, and very easily.

A common pound phial of green glass, that contained about thirty-eight square inches of coated surface, which I then had, when perfectly dry and clean as I could make it, would discharge spontaneously with a charge of about fourteen or fifteen grains of repulsion; but I found I could alter it, with respect to its cleanliness, so as to take a charge whose repulsive force should be equal to twenty-four grains.

In

In a jar that I then had, which contained about sixty-four square inches of coating, the jar being seven inches high and four in diameter, I found the difference much greater; for, when quite dry and clean as I could make it, it would discharge spontaneously with five and a half grains; but by altering its cleanliness, that is, by dirtying the naked part of the glass*, I made it take a charge of thirty-five grains, which is six times as much as when quite clean: and it would not then discharge of itself, but ran over the top, all round the jar, as water would have done by being over filled, and is very conspicuous in a dark room.

In regard to the application of the silk, I owe all hints towards it, to Mr. Ferguson: for I had not seen, nor did see, till six months

* The Rev. Mr. Morgan having told Mr. Nairne of the great difference of charge, which I had found, that a bottle would take, when the uncoated part of the glass was a little dirtied, or not quite clean, Mr. Nairne, somewhat facetiously said, that I was fortunately situated, for it was only the *dirt of Norwich* that would do so. Mr. Nairne seems to me to be one of the best electricians that I have met with,

after,

after, either Dr. Franklin's Book on, or Dr. Priestley's History of, Electricity; where I then found that the former had tried a piece of leather something in the same way, but he did not make much of it.

From the many electrical experiments that I was making in the year 1771, it plainly appeared, that a certain quantity of surface disposed in length, and joined to a prime conductor, added much more to the strength, or pungency of a spark, or stroke, than the same quantity of surface disposed in thickness, or breadth, and joined to the same prime conductor.

I had an iron rod five feet long and about three-eighths of an inch thick, with a ball three inches diameter at one end; the other end of it was held in the prime conductor: I likewise had a conductor, made of paste-boards, and covered with tin-foil, about nine inches diameter and twenty-five inches long; but the latter, being joined to the
prime

prime conductor, gave a spark which was by no means equal to the former, although the difference of the quantity of their surfaces was so very great. If the hand were shut close, and a spark were taken on the back of the hand from the prime conductor, having the iron rod joined to it, the whole hand would be convulsed, and if I stood on a ground floor, I felt every spark, or stroke in my feet. I mentioned this to Mr. Bewly, May 20, 1771, but he gave me for answer, that he somewhat doubted the fact, and thus this part of the business was then dropped.

Some time in or before the year 1778, the Rev. Mr. Morgan read some Italian publications to me; one of which contained an account, given by Sig. Volta, of a number of *Bastions*, as he called them, or rods, eight feet long and half an inch thick, insulatedly suspended, in different positions; and finding this had some resemblance to my former observations, I reassumed the subject.

Having

Having by me a round iron rod near ten feet long, and fifteen eighths of an inch in circumference; I made another rod, of fir, the size and length of the iron one, and covered it with tin-foil. A ball about three inches diameter being put on at each end of the rods, made each rod, with its balls, ten feet long. These were tried separately, to see if any sensible difference could be discovered in the iron rod, from that of fir covered with tin-foil: but one appeared to give a spark, or stroke, equally strong as the other. I next tried them together, one joined to the end of the other; thus they gave a much more powerful stroke. Having these two rods separately insulated, I next placed them parallel to each other, at the distance of between three and four feet, and made a communication between them, to observe the strength of the stroke they would give in that position. After this they were removed to about ten or twelve inches distance, in order to examine if there were any perceptible difference in the strength of the stroke when they were near, or further off,

off, each other. The former, Sig. Volta speaks of, as a very considerable hinderance, by the interference of their atmospheres; but I could find no sensible alteration.

I think I observed something of this kind, done very lately; in Mr. Adams's Book on Electricity; or rather, to see if there were any difference between a solid body and a hollow one of the same external dimensions, but none was found.

One of my rods contains about two hundred and thirteen square inches of surface. I have a prime conductor about four inches diameter and near three feet long, which contains about four hundred and sixty-eight square inches of surface; but the latter, joined to my common prime conductor, does not give so powerful a spark by much, I mean as to sense, as one of my rods joined to the same common prime conductor, although the surface is more than double.

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After

After trying a rod of wood and a rod of metal, and finding no sensible difference, I made twelve rods, the size of the former, of fir, covered with tin-foil, and put a ball on at each end about two inches and a half diameter, which made each rod, with its balls, about five feet and a half long. Having placed my former two rods at the distance of between three and four feet, nearly parallel to each other and nearly the same height, I laid the twelve short rods nearly equidistant from each other upon the two long rods, so that they were about ten or twelve inches asunder. Then, upon making a communication from the prime conductor to any part of the rods, and working the machine to electrify, or rather to charge them, they would give a stroke much too severe to be often taken: infomuch that (these strokes, quickly taken, so affecting the legs and ancles, with a powerful machine, such as Mr. Nairne's large ones are, to charge the rods quick enough withal) I believe they would soon bring a very strong man down, if he stood on a ground floor.

These

These rods I suspended by glass sticks about a yard long. About the middle part, from the underfide of the rods, I suspended (on points, in order to move freely) another rod, about six feet long, with a ball of lead at one end, and a large cork ball covered with tin-foil at the other end, and a Florence wine flask covered with tin-foil likewise, and suspended freely close to the cork ball: consequently the center of gravity was much nearer the leaden ball than the middle of the rod, and the end of the arm with the flask, on the opposite side of the center of gravity, was much the farthest off. The whole of this additional apparatus being suspended under the former rods, and the whole charged, or electrified together, the flask will be attracted by whatever is nearest to it, either sideways, or below it. Let a thunder-house be placed any where lower than, and in the reach of, the flask, or, instead thereof the spire of a cathedral in miniature, with an interrupted communication to the ground, and a ball on the top of it, the flask will approach the ball

and discharge its electricity upon it, and beat it down, without any phial or jar to assist it; but if the spire terminates in a point, and the communication to the ground be complete, every thing will be safe.

If these rods can be supposed to represent a thunder cloud in the air, and the moveable flask to represent the tail, or an inferior part of the cloud, which may be driven about by the wind, or attracted, if charged with lightning; I say, if these suppositions can be admitted, I think this is the most natural representation thereof that I have yet heard of: and at the same time plainly shews, which is to be preferred for the termination of conductors to preserve buildings in thunder storms, or from lightning, points, or balls.

With regard to excellence in electric machines, as to their construction, and power, I have never seen any, of its size, equal to *Mr. Nairne's* patent one; and much of the apparatus is incomparable, particularly for medical purposes;

purposes; though at the same time, the machine is applicable to every other purpose where electricity is concerned.

Whatever may be the size or form of an electric machine; it seems that the general aim in making it, is to have its construction such, as that it may collect, or produce, the most electric fire possible with the least labour: but at the same time I think it is as indispensably necessary, that its construction be such, that after the electric fire is collected, or produced, it may be prevented from being dispersed, or escaping, by the admission of conducting substances into the atmosphere of the machine, or the prime conductor. It also seems to me that for the purpose of charging of batteries, a large prime conductor is very disadvantageous: also, that it is highly necessary for the battery to be placed at such a distance from the machine and the prime conductor, as to be as much as possible out of their atmospheres: and if the machine and the prime conductor, be large, the operator ought to stand insulated; otherwise,

otherwise the atmosphere of a large apparatus will extend so far round, that a great deal of the collected electric fire will escape, or be dispersed by him into the floor on which he stands.

When that excellent set of electrical experiments were made by Mr. Nairne, which are inserted in the Philosophical Transactions VOL. LXVIII. he seems to have been well aware of these circumstances in the construction of his apparatus; and if I am rightly informed, the operator, whilst he wrought the machine, stood insulated.

Amongst the various conjectures relative to the cause of the *Aurora Borealis*, electricity has been considered as a principle agent in producing these appearances; and in order to verify these conjectures, I have, at various times, even whilst these appearances were the greatest, put up my insulated conducting rod, to observe whether or not it would be affected: but the cork balls in my room, connected with the rod, seemed not to diverge any

any more at those times than when there were no such appearances, nor indeed to be affected by any electricity at all. More observations made at different places by different persons, may probaby cast further light upon this affair.

N. B. Respecting the degree of foulness of the naked part of the glass already recited, in order to give the greatest charge to a Leyden Phial, &c. that it is capable of; it seems necessary, that something of a very slight oiliness be rubbed over its surface, or something, nearly a non-conductor, which will adhere to the glass very thinly; such as the perspiration of one's hand, when it *begins* to be in a state of perspiration, from exercise. If the electrical apparatus be kept in a very warm and dry room, the advantage of the above method of foiling the naked part of the glass will soon appear; but if the apparatus be kept in a cool room, where neither a fire is kept, nor the sun enters, the aforesaid method will be of very little use. What there is in the composition of the electric fluid, that some part of it should run over the top of the jar and the jar not discharge, and that some other part of it should stay behind and keep the jar so highly charged, as recited in page 101, I cannot even give a hint at: but the running over does not begin till the jar is very highly charged, and then it is attended with a considerable noise, almost like very strong hissing.

CHAPTER IV.

OBSERVATIONS

ON THE

AIR PUMP.

AFTER having so much improved my electric machine by the application of the silk, it acquired so much approbation, that many Gentlemen desired me to send them a new machine on the same plan as my own, with apparatus to them; amongst which was the exhausted, or luminous flask. These flasks I procured from London, till I had some difficulty to get them readily; which brought on so much inconvenience, that, as I had no air pump, I determined to make one, of the

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table

table kind, which, when compleated, proved to be a very good one. With this I made many luminous flasks, where the difference of exhaustion plainly appeared, by the time that they would continue to be luminous; and those that were luminous the longest were those that were the most exhausted, consequently, on some occasions preferable: which made me desirous of exhausting them still more, if possible.

The accounts given of Mr. Smeaton's Air Pump, put me on enquiring of many Gentlemen, &c. about them, in order, if possible, to procure one for trial. One of those Gentlemen was the Rev. G. Walker, F. R. S. who gave me for answer, that he had one, and I should have it to try*. Very soon after this, he came himself from Nottingham, where he then resided, and brought his pump with him. We took a time for trial of it, together with my own pump; when several other Gentle-

* I informed Mr. Bewley of this on the 25th of July, 1776.
men

men met us, and we gave them a fair trial: but could find no difference between his and my own, in respect to the power of rarifying. When it was in the best order we could make it, we could bring the same gage, with either pump, down to about four lines and a half, but no lower. By this it plainly appeared, that the Smeatonian Pump was not altogether what it had been represented: but they were both used with wet leathers. Having found that either pump would do the same with the same gage, it occurred that either pump might appear to do differently with a different gage, that is, one something like Mr. Smeaton's, or the pear-gage. And having read Mr. Smeaton's account in the Philosophical Transactions, Vol. XLVII, I determined to put his descriptions into practice as well as I could: and in order thereto, I made a glass similar to his for a gage, but something longer. The tube part of the gage was six inches long above the bulb; and the size of the bore was such, that the weight of the mercury, which filled three inches length, was five penny-

weights, nine grains and a half. I exhausted this glass, over a cup of mercury, set upon the air pump, under a common gage glass receiver, so that all the new gage was above the mercury. When it was exhausted nearly as far as I could, I let the gage down into the mercury, and opened the pump to fill the gage by the pressure of the common air. When it was as full as it would fill, I marked the glass tube as it stood erect (having taken off the receiver) with a file, at the top of the column of mercury. After this, I took out the remaining air at the top of the gage, by the help of a wire, and filled the whole gage as full as I could with mercury: and having emptied the gage, I weighed the mercury which it contained, and found its weight, eight ounces, fourteen penny-weights, and twelve grains. Then I filled that part of the gage which was above the aforesaid mark (made with a file on the gage) with mercury: and weighing this portion by itself (as I thought this a more certain method than trusting to divisions on the gage) I found that by this new
gage

gage, my pump appeared to rarify near two hundred times, and that this gave me a much greater number of times rarifying than my common gage, which I had on the pump at the same time. This apparent difference made me desirous to repeat the experiment, and to be as exact and careful about every particular as I could. Accordingly, I then new valved and leathered my pump, and made the experiment again; and found five hundred and fifty-one times rarefaction.

The two foregoing experiments falling out in this manner, made me determine to try tubes of various sizes and lengths, joined to the same bulb, one after another. Accordingly, I cut my present new gage asunder at one inch above the bulb; and to this same bulb and short part of the tube I joined, with cement, another tube twenty-five inches long. I also lengthened the tube part of my gage glass, for a receiver; and on proceeding with this as with the former, it gave me sixty-seven times rarefaction. Three inches of this bore

bore contained three penny-weights, thirteen grains, and a half of mercury.

EXPERIMENT III.

I then separated this long tube from the bulb, at the place where they were joined, and to the same bulb as before, I joined another tube fourteen inches long; and on repeating the former processes, it gave three hundred and fifty times rarefaction. Three inches of this tube held four penny-weight, two grains, and a half of mercury.

EXPERIMENT IV.

Having separated this fourteen inch tube from the bulb at the juncture, I joined another tube five inches long; so that my gage was thus the same length as the first, but the tube was smaller: and on trial as before with the others, this gave eight hundred and ninety-eight times rarefaction. Three inches of this tube contained one penny-weight and twenty-three grains of mercury.

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EXPERIMENT V.

I here took the piece of tube which with the same bulb made the first new gage, and cut it afunder at two inches from the top of it: this piece of two inches I joined to the aforefaid bulb; and found by *this*, that my pump rarified nine hundred and two times. The size of the tube is given with the first trial.

EXPERIMENT VI.

My next trial was with the last mentioned tube but one; which I also cut afunder at two inches from the top: and having joined the piece of two inch long to the former bulb, as before, I found that by this short and small tube my pump rarified one thousand five hundred and ninety-eight times. This is a piece of the tube, of which three inches held one penny-weight, and twenty-three grains of mercury.

The above experiments are numbered, as beginning where and when the pump was new valved, &c. and the fractional parts of rarefying are omitted, as being unnecessary.

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It appears that Mr. Smeaton's gage was not quite four inches long, including the bulb and tube together: and he says that the gage contained about half a pound of quicksilver, and that a thousandth part thereof would fill about one-tenth part of an inch in the tube-part of his gage. On the whole it appears that the tube-part of his gage was much shorter than mine; so that if my gage had been as short as his, my pump might have appeared to rarefy as many times as his: for the length as well as the size seems to be of considerable consequence; and more so, if I had used the method that Mr. Smeaton used, that is, to turn the gage horizontally before he took the space between the top of the column of mercury and the top of the tube. Taking the space in his manner, would have indicated a much greater number of times rarefaction, than the manner I used (as I found by trial) consequently much further from the truth.

I think that if the principle, on which this gage is supposed to act, were well-grounded,
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the gage would speak the same language in all cases: but that it does not, is very plain, or I am much mistaken.

The principle on which I set out, on the subject of rarefaction, is to suppose a common torricellian tube well filled with mercury, and purged of air as much as possible, placed erect in a cistern of mercury: let the tube be about thirty-two inches high above the mercury in the cistern; and the mercury in the tube will fall to a height, which will be an exact balance to the common atmosphere: and placing a receiver over the whole upon an air pump, and exhausting the air out of the receiver, the mercury will subside in the tube so as to be always a counterbalance to the pressure of the air, which acts upon the surface of the mercury in the cistern. Thus, I think I may say, as the space through which the quicksilver in the tube has fallen, is to the space through which it has to fall (to be level with the surface of the quicksilver in the cistern) so is the degree of exhaustion, or

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the quantity of air taken out of the receiver, to the quantity remaining in it*.

Mr. Smeaton says (speaking of his valve) that in the common pump, *'the valves are stretched.'* I believe that if the valves in any pump be properly put on, they will rise much easier than his; their weight, to be raised, may be much less: and the places of cohesion round the holes may be much less: for the

* If this principle be not a true one, I am deceived, but I believe it will hold good at all times, and in all places. And in all the foregoing experiments, with the pear gage, I believe the utmost degree of rarefaction was very little more than fifty-nine times, having another gage in the pump all the time, as before observed.

If in a torricellian tube, well purged of air, the mercury stands thirty inches high, and if with an air-pump it can be brought down to half an inch of the level of the surface of the mercury in the cistern in which the tube is placed; I think it may be said, that the mercury is brought down fifty-nine parts out of sixty; and consequently, that fifty-nine parts out of sixty, of the air that was in the receiver on the pump, is taken out; and that the air in the receiver is but one sixtieth part so dense as the common air; or in other words, that the air in the receiver is rarefied fifty-nine times: and that pump which will bring the gage the lowest, I should readily allow to be the most perfect.

valves

valves will lie plain on the *fides* over a small hole much better than over a larger one, when a power to press them in is applied: and when this is the case, if the valve should rise any sooner, in the middle part, and the air in a rarefied state be not of force enough to raise the outermost part of it, this being where the greatest cohesion is, and, as aforesaid, will not rise so soon as a valve in a common pump may be made to do*; the pump may be kept working, and no air will come out, continue the working ever so long:

* To make a valve for an air pump, I should recommend a strip of very thin bladder, about five-eighths of an inch broad, made very pliant by rubbing it in the fingers with oil, then fixed down close as possible; by *stretching* it over the hole that leads to the bed of the pump; a slit to be cut at right angles to the strip on each side of the hole, about one-fourth of an inch distant from the hole with a sharp knife, or the like, held a little leaning from the hole, and some oil put upon it. By this method I think a valve may be made to rise the easiest, and be the most effectual of any that I know of. Thus, instead of hindering, the *stretching*, is very serviceable: or in the manner of Mr. Bewly's pump made by Mr. Nairne, where four slits may be cut: in either, the valve will be held by four places, as well as Mr. Smeaton's, though much more easily raised.

therefore, how can any advantage be expected in or from the valve?

Having informed Mr. Bewly, that I was to have a Smeatonian Pump sent me for the purpose of trying it, he wrote to me on the 17th of August 1776, as follows: 'When you last wrote to me you told me you were going to review an air pump of Smeaton's construction. I shall be much obliged to you if you will inform me of the result of your examination.' Accordingly on the 19th of the same month I sent the result to the purport as in pages 114 and 115. Also in the same letter I wrote to him thus: 'If Dr. Priestley would send his pump to Norwich for us to try, I would willingly pay the carriage hither and back again. Do you think it would be a fair question to ask him? Such properties as he speaks of are very desirable in a pump; but if he is under any mistake about it, I should think he would like very well to be set right: let me ask the favour of your answer.'

Mr.

Mr. Bewly did not write to me any more till the 31st of August, 1776. But on the 27th preceding, as I had been making further trials, I wrote to him again, informing him, ' that I thought I had found out the mystery ' by which Mr. Smeaton's pump and gage indicated such great powers and degrees of rarefaction: and that with my own pump and ' his gage I could, without any sort of difficulty, rarify fifteen hundred times, and much ' more.' Also I informed him, in brief, of some results which inclined me to think so; and that, if it were worth his knowing, I would send him the particulars.

On the 31st of August 1776, as before observed, Mr. Bewly wrote to me again, as follows, ' I am much obliged to you for the information I received by your letter, relative to the Smeatonian gage. Though you ' say nothing of the cause of the deception you ' have discovered; yet I think I can make a tolerable guess at it.' He likewise tells me that,

' another

‘another Gentleman* with himself had just
‘been talking of this gage. It occurred that,
‘on the mercury’s rising into the gage, it must
‘probably entangle a great part of the in-
‘cluded rarified air, particularly against the
‘sides of the glass; so that the air which ap-
‘pears in the upper part of the tube, and
‘which Smeaton considers as being the *whole*
‘that had been left in the gage, on the ex-
‘haustion, may only be a tenth part of it;
‘the remainder being dispersed over the inner
‘surface of the large globular part of the
‘gage. Pray inform me whether this is not
‘the case. I should be glad likewise to re-
‘ceive the particulars.’

Though Mr. Bewly had no opportunity to
make his conjectures perfectly satisfactory; yet
I think it will appear in the sequel, that they
were well founded.

In the same letter he adds, in very strong
terms, tending almost to a degree of wonder,

* The Rev. Mr. Brand.

or

or in the height of surprise, ' The only difficulty that strikes me, is how Mr. Smeaton and Dr. Priestley could deceive themselves so egregiously in this matter. Did they never use the *common gage* along with this? If they had, and if the pump had actually rarefied the air sometimes even 2000 *times*, or even 1000, the mercury in the common gage must have been brought down to a level,—I mean as to sense, with that in the cistern. I do not find however that they any where assert this. Could they wilfully shut their eyes, and believe the air was rarefied to 2000, when the mercury was at the same time standing in the common gage at the height of three or four lines at least? I know not how to reconcile these things.

' Not two hours before I received your letter I had sent one off to Dr. Priestley, in which I had informed him of the results of your examination of a Smeatonian pump, and of your doubts and difficulties. I wish I had had your letter before this was sent off; but if you will

‘ will enable me to be more particular, I will
‘ give him a full account of the matter.’

After these requests of Mr. Bewly’s, I sent him the particulars on the 9th of September 1776, nearly as recited in the last six Experiments, either for Dr. Priestley’s, or his own use; to which he gave me an answer on the 21st of September 1776, In the following words: ‘ I return you many thanks for your gages,’ [I had previous to this, sent Mr. Bewly several pear-gages,] ‘ and your epistolary communications, ‘ which satisfactorily prove the fallacy of the ‘ *Smeatonian* gage; though you will find below, ‘ that Dr. Priestley speaks with confidence ‘ of the great powers of the pump of that ‘ name.’

‘ When I wrote first to Dr. Priestley on the ‘ subject of Smeaton’s Pump, I could only ‘ inform him, in general that you had found it ‘ in no respect superior to a good common ‘ pump, in the article of rarefying. Here is ‘ what he says on the subject.’

‘ The

"The pump which Mr. Brook examined must have been a very bad one, indeed. Mr. Nairne has improved upon Mr. Smeaton. Before I left London, I saw one of his exhaust near ten thousand times."*

'Surely, all these good people, though they may make a mistake of a few thousands in estimating the power of the Smeatonian pumps, in particular, must be able at least to rarify a few hundreds.

'Since you informed me of your experiments made with Smeaton's gage, I have, in general, informed Dr. Priestley of the results, and have put some questions to him on the subject. As soon as I hear from him, I will communicate to you his answer, or am informed whether these potent rarefiers of air, to 10,000 and upwards, have any other collateral evidence to produce, to strengthen the suspicious testimony of the Smeatonian gage.'

* Mr. Nairne is one of the best Philosophical Instrument-makers I ever knew.

S

Hitherto

Hitherto Dr. Priestley appears to be pretty firm in his opinion, that the Smeatonian apparatus has great superiority, notwithstanding what Mr. Bewly had informed him of: but now he soon finds his mistake.

On the 7th of October 1776, Mr. Bewly sent me another letter, in which he writes as follows: 'I received a letter from Dr. Priestley last post: the contents of which, so far as they relate to the Smeatonian pump and gage, you will be curious to know.

'In a former letter he had told me that he never had used a gage in which the mercury had been boiled; and that he knew not how to execute the process. I described it to him in the most minute and circumstantial manner; desiring him to prepare a simple gage after this manner, and then try whether his *Smeaton's pump* would bring the mercury contained in it within a *line*, or less, of the mercury in the bason; as it ought to do, if it rarefied the air *only* 3 or 400 times. He has
'tried

‘ tried the experiment; and the following is the
 ‘ result:

‘ His pump was foul, not having been
 ‘ cleaned for several months past; “ *and his*
 ‘ *common syphon-gage shewed that it did not ex-*
 ‘ *haust near so much as it had formerly done.*”
 ‘ He was content with it nevertheless, and in
 ‘ this state, he included both the Smeatonian
 ‘ gage and the boiled gage in the receiver, at
 ‘ the same time. On exhausting, Smeaton’s
 ‘ gage gave him 300 degrees of rarefying
 ‘ power; while the mercury in the boiled gage
 ‘ fell down only to a little less than *half an*
 ‘ *inch* above the level; and accordingly indi-
 ‘ cated only about 60 degrees. ‘ He would
 ‘ have cleaned his pump, he says, had he not
 ‘ been convinced that what he had already
 ‘ seen was sufficient to discredit the Smeato-
 ‘ nian gage. He observes that “ *you will gain*
 ‘ *great credit by the detection;*” and that the gage
 ‘ is certainly fallacious, though he does not
 ‘ comprehend the cause of the fallacy.’

Notwithstanding Dr. Priestley already appears to be so perfectly convinced, that the Smeatonian pump and gage were not what they were supposed to be; he does not content himself with informing only Mr. Bewly of it, but, with his uncommon frankness, without staying any longer, on the 20th of October 1776, he writes immediately to me as follows: 'You have great merit in the detection of the fallacy of Mr. Smeaton's gage: and I wish you would draw up a regular account of your observations on that subject. It is of great use to know what we have to depend upon in business of such importance in philosophy. Mr. Nairne, who makes the best pumps of Mr. Smeaton's construction, has heard of your objections to them, and will soon, I believe, give Mr. Bewly an account of what may with certainty be expected of them. I intend soon to write to Mr. Smeaton himself on the subject, and you shall know what he says if I have any answer.'

Accordingly

Accordingly I received a letter from Mr. Bewly, dated November 24, 1776, in which he writes as follows: ' By a letter received from *Mr. Nairne* yesterday, I find he is to send me next week a pump with which he has taken particular pains*, accompanied with an account of some experiments relative to the subject of exhausting. Such experiments as I can make with my present instruments, I will take an early opportunity of making, and inform you of the results.'

The above-mentioned account of some experiments is dated Nov. 22, 1776, which by *Mr. Nairne's* permission, Mr. Bewly favoured me with to peruse, on the 30th of January, 1777; and they appear to be the first that *Mr. Nairne* had communicated to Mr. Bewly, and likewise some of the first that he had made on this part of the business, as will more fully appear afterwards.

* Mr. Bewly afterwards sent me this pump to try; and I pronounced it one of the best I ever saw.

Mr.

Mr. Nairne says, in a letter to Mr. Bewly, dated August 15, 1777, 'I cannot conceive why Mr. Brook should think that my trials originated from any experiments I had heard of his, either by his letter to Dr. Watson or yours to me. If he has Mr. Smeaton's letter to Dr. Watson in answer to his, he will find, I shewed Mr. Smeaton, Hon. Mr. Cavendish, Mr. Aubert, and some other Gentlemen, the great difference between the two gages, &c. in April 1776, (viz. above 2000 times by the pear-gage) whereas his letter to Dr. Watson is dated Nov. 16, 1776*, and your letter to me is dated Sept. 7, 1776, wherein you first mentioned Mr. Brook. Before I received that letter of yours I had never heard that there was such a person as Mr. Brook, that made air pumps or that tried experiments on that instrument.'

* My letter to Dr. Watson was selected from the experiments I made immediately after the receipt of Mr. Bewly's letter, dated Aug. 31, 1776, and communicated to him Sept. 9, following. But Mr. Nairne seems here to be only preparing to make those experiments which were communicated to Mr. Bewly on the 22d of Nov. 1776, in consequence of his having heard of *my objections*, according to Dr. Priestley, see page 132.

Hence

Hence it appears, as if Mr. Nairne were desirous to date the beginning of his trials to find out the deception of the Smeatonian gage, from the time he first discovered the difference between the two gages, which was in April 1776. But though he then discovered a difference between the two gages, it does not appear that he did any thing more about it, till he made the experiments which he gave an account of to Mr. Bewly on the 22d of November, 1776: but that all the time from April, 1776, till about the time Dr. Priestley informed me that Mr. Nairne had heard of my objections, the business lay dormant. But neither then, nor afterwards, does it appear, that either himself, or Dr. Priestley, found, or even supposed, any deception in the Smeatonian pump as well as the gage: whereas it was the deception of the pump alone, that first occurred to me, not having any pear-gages to try. But it plainly appeared to me, before the 19th of July, 1776, that the pump was fallacious [see pages 114 and 115] and also that I had discovered the gage to be

be equally fallacious as the pump, before the 27th of August, 1776, and that I communicated an account of both to Mr. Bewly at the times already specified. Likewise it is plain, that Mr. Bewly gave an account thereof to Dr. Priestley on the 31st of August, 1776. And it is not less plain, that Dr. Priestley was well satisfied, that there was a fallacy, before the 7th of October, 1776, and that Mr. Nairne, *had heard* of my objections to them before the 20th of October, 1776, and about that time set about those experiments dated Nov. 22, 1776.

Mr. Nairne's accounts and experiments are printed in the Philosophical Transactions, Vol. LXVII. But if any claim is to be laid to the discovery of the fallacy of either the Smeatonian pump or the pear-gage, I think I have the greatest: the former of which he does not seem even now to suppose. However, I think it will plainly appear in the sequel, that any tolerably good common constructed air pump may be made *to appear* to exhaust equal to any on Mr. Smeaton's construction,

tion, with the same gage to both; consequently that the supposition relative to the excellency of the construction of his pump in the article of rarefying, is also fallacious.

Mr. Smeaton speaks of his construction, as being easier to fit the bottom of the piston close to the place where the valve is fixed, that leads to the bed of the pump. But I think this supposition is not less fallacious than the supposition of the excellency of the whole pump, particularly in this respect. For I can see no cause, or reason, why the piston cannot be made to fit as close at the bottom of it, in a common constructed pump, as it is in one of his construction: I can see, or find, no difference in either. If I could suppose any, it would be in his, as being the largest in diameter, and not less difficult to fix at right angles to his rod. In short, the only circumstance of use in Mr. Smeaton's pump, by which any convenience or advantage is gained over the common construction, is *condensing*; all the other advantages being only suppositions.

T

tions. Indeed, I think such as Mr. Nairne makes, of the table kind, are very much preferable.

Mr. Smeaton speaks of using water with his pump; not then knowing how much it favoured the deception of the pear-gage: which will plainly appear afterwards.

On the 16th of December, 1776, Mr. Bewly sent me an abstract of Mr. Nairne's account of some experiments, dated the 22d of November, 1776, in which the pear-gage was used with the common one. This put me on making more pear-gages, (six) with which I made many experiments, and repeated them many times: but, as they were not very materially different from Mr. Nairne's, I shall insert only three of them.

EXPERIMENT I.

A pear-gage, the tube of which was six inches above the bulb, three inches of it containing six penny-weights two grains and a half

half of mercury, and the whole gage containing seventeen ounces, ten penny-weights, eleven grains, with the apparatus all dry, indicated one hundred and twenty-seven times rarefaction, with or without box-wood* in the receiver.

EXPERIMENT II.

A pear-gage, whose tube was three inches long above the bulb, and held one penny-weight and twenty-three grains, and the whole gage eight ounces, six penny-weights, and seventeen grains of mercury, with dry apparatus, gave two hundred and four times rarefaction, with or without box-wood, weighing one hundred and seventeen grains.

* Mr. Nairne found that a piece of box-wood, weighing one hundred grains, included with the pear-gage in the receiver, made a great difference: but I could find none, though cut from different logs, but all pretty dry.

EXPERIMENT III.

The last gage, under the same receiver, (the receiver, instead of being cemented to the bed of the pump, being set upon a piece of wet leather) indicated one thousand three hundred and thirty-three times rarefaction.

Here the effect of the water, in regard to the deception of the Smeatonian gage, is pretty obvious, [see page 138.] It is not less obvious that a common pump may be made to rarefy as much as a Smeatonian one will [see page 136] though the apparent difference of rarefying is above eleven hundred times.

In order that there might be no moisture in the way, previous to these three experiments, I cleaned and refitted my air pump, at the same time warming it by the fire, that the oil and valves might be as thin and pliant as possible; so that I could bring the boiled gage down to five-sixteenths of an inch; which, if the barometrical weight of the atmosphere be
equal

equal to a column of mercury thirty inches high, indicated a rarefaction of ninety-six times. I likewise made two new plates to my air pump, of thick plate glass; one for the bed of the pump, and the other to set the gage upon: and instead of using wet leathers to set the receivers upon, I cemented them down to the plates of glass, with a cement of turpentine, bees-wax, and tallow, with a little red lead to colour it; so that the pump was used without any water, or wet leathers.

These experiments, together with those not here inserted, which I noted down, amounting to about forty, were made after December 16, 1776, and communicated to Mr. Bewly, Jan. 30, 1777.

The following is a set of experiments with different Smeatonian, or pear-gages, in which the mercury was *boiled* in every part of them when so specified.

EXPERIMENT

EXPERIMENT I.

A pear-gage, the tube part of which was six inches and three quarters above the bulb, three inches of the bore holding five penny-weights and nineteen grains, and the whole gage containing fifteen ounces, ten penny-weights, and twenty-three grains of mercury, gave two hundred and ninety-nine times rarefaction. This was boiled before I began the experiment.

EXPERIMENT II.

The last gage new filled and boiled, and with a piece of sheep skin dressed with allum, which weighed one hundred and ten grains, inclosed in the receiver with the gage, now gave one thousand, eight hundred, and sixty-five times rarefaction.

EXPERIMENT III.

The same gage as before, unboiled, and without the sheep skin, now gave one hundred and seventy-five times rarefaction.

EXPERIMENT

EXPERIMENT IV.

The former gage, not boiled, but with the sheep skin, now gave fourteen thousand nine hundred and twenty-two times rarefaction.

EXPERIMENT V.

With a pear-gage, the tube part of which was five inches above the bulb, and three inches of the tube held three penny-weights and two grains of mercury, and the whole gage contained six ounces, eighteen penny-weights, and thirteen grains. This gave four hundred and seventy-seven times rarefaction, unboiled and without the sheep skin.

EXPERIMENT VI.

The last gage, with the sheep skin, and not boiled, but without any other known difference, now gave a number of times rarefaction not to be ascertained with any precision whether twenty, fifty, or an hundred, thousand times, and with as much propriety one as the other; for the space left vacant did not appear
to

to be larger than about half the size of a mustard-seed; though the upright boiled gage was not sensibly different from all the former, which was each time brought down to three tenths of an inch, and indicated one hundred times rarefaction.

These experiments seem more fully to prove, that a common pump may be made to rarefy as much as any one of the Smeatonian construction, as already recited.

EXPERIMENT VII.

In the time that I was making the foregoing experiments, I tried many other pear-gages in which I boiled the mercury: but as the results of many of them were similar to the first two, I did not note them down. Many of them were so well boiled, that the mercury was suspended to the top of the tube at different distances: and notwithstanding I used violence to shake it down, in some of them the cohesion was so great, that I could not get it down at all till I opened the pump.

Although

Although it was contrary to my inclination, not to get all the mercury down in the tube, I am now inclined to think that this was a favourable part of the business. And although the upper part of the column could not be shaken down, yet it would not materially alter the experiment. For in all these experiments with the boiled pear-gages, I inverted the open end of them into a cistern of mercury before I began to exhaust: and when the pump was exhausted as much as I could, I raised the open end of the gage out of the mercury in the cistern, to let out the mercury in the gage, and to let the rarefied air enter: and then nearly the same space between the two ends of the columns of mercury in the tube would be unoccupied, as if the whole column had come down, and the unoccupied space had been at the top of the tube.

After the gage was filled in the Smeatonian manner, I took out the air between the two columns, and let them join together; after which, although the open end of the gage was

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not

not lifted out of the mercury, till the pump was exhausted as much as I could exhaust it, and then only raising it out and pushing it down into the mercury again as quick as I could, there was so much vapour or damp, &c. that had taken place on the inner surface of the tube, that after I had taken the gage out of the receiver, the place of the juncture was visible to the greatest exactness; whereas had the whole column of mercury came down at first, I should not have been able to discover this coating of vapour, &c. on the inside of the tube.

EXPERIMENT VIII.

I went on to try several gages in this manner, that had a part of the mercury remain, or suspended, at the top of the tube, and always found this covering with damp, or the like, to take place, when the pump was exhausted as much as I could, in gages that had been well boiled: and had they not been boiled, none of the mercury would have remained at the top of the tube.

EXPERIMENT

EXPERIMENT IX.

Proceeding on a supposition that I might not so perfectly boil the mercury in the bulb part of the gage as in the tube part thereof; and that if any air, &c. was left in any part, I might discover it by exhausting the receiver to let the mercury subside into the cistern out of both tube and bulb, and letting it up again, without taking the open end of the gage out of the mercury (this gage also having part of the column suspended at the top) by letting the mercury up into the gage to fill it as it stood, only a very small speck was discoverable at the joining of the mercury in the tube: so that the bulb, as well as the tube, which together held nine ounces and seventeen penny-weights, appear to have been very well boiled.

EXPERIMENT X.

Finding no damp, &c. to appear in the tube by filling the gage under the receiver, after the mercury had been let down, and

U 2

letting

letting in the air to fill the gage by its pressure, without taking the open end of the gage out of the mercury; my next trial was with a pear gage, the tube of which was five inches above the bulb, to see if there would be any damp, &c. lodge upon the inner surface of the gage, by emptying part of it and filling it again in open air: and having boiled the gage, I exhausted the receiver, with the gage in it, to let the mercury subside out of the gage into the cistern; and then filled the gage again by the pressure of the air, without taking the open end of the gage out of the mercury. This, also, having part of the column suspended at the top of the tube, as before, exhibited the aforesaid speck at about half an inch from the top. In this I took the gage out of the receiver, and emptied the bulb and about half the tube, and filled it again in open air: but now not the least more of this coating, &c. appeared in the tube than what appears in every clean tube that has been filled with mercury and not boiled. I repeated this often; and the result was the same.

These

These appearances, I think, seem strongly to indicate, that this coating, or covering, with damp, or vapour, &c. takes place only where the air is in a rarefied state, or at least, most when the air is highly rarefied. I believe it is generally allowed, that water or damp absorbs air very copiously. Thus there may be no great difficulty to suppose the aforesaid coating, or covering with damp, vapour, or the like, to do the same: and admitting this, the difficulty of accounting for the irregularities and deceptions of the pear-gage ceases at once. In this case also, not only the absorbency of the damp, &c. may be so favourable to the deception in the pear-gage; but if any of these tubes thus covered on the inside with damp, &c. are examined with a magnifying glass, they will appear like any other glass that has been breathed upon, affording numberless vacuities and lodgments in which the mercury will entangle the rarefied air all the way as it rises in the gage. This verifies

Mr.

Mr. Bewly's suggestions in a very high degree*. [See page 126.]

Notwithstanding all this difference in the pear-gage, the upright boiled gage was not sensibly affected, which was each time brought down to three-tenths of an inch. And I believe this to be the most accurate measurer of air rarefied, or condensed, to any degree of which the apparatus is capable.

In the abstract of Mr. Nairne's letter which Mr. Bewly sent me on the 16th of December, 1776, he mentioned a great difference that Mr. Nairne found, by including a piece of box-wood in the receiver with the pear-gage: after which I made some experiments including a piece of box-wood with the pear-gage in the receiver; but found no difference.

* The Honourable Mr. Cavendish seemed to have formed an idea something similar to Mr. Bewly's, as related in Mr. Smeaton's answer to my letter to Dr. Watfon: which is noticed afterwards.

[See

[See note, page 139.] However, after I informed Mr. Bewly of it, he wrote, by way of note in a letter, dated January 29, 1777, as follows: 'Though you made your apparatus as clean and dry as possible; yet you could not probably clear it of the humidity contained in the inner works of it. If so, there might be no alteration, whether the box-wood were under the receiver or not.' I think it seems as if he thought my pump did not rarefy far enough to bring on the difference with the box-wood; which put me on making some more experiments with the pear-gage, to see what degree of exhaustion is necessary to make a difference appear: and in order thereto, I made two new pear-gages of very different sizes.

EXPERIMENT XI.

One of the above gages, the tube part of it being three inches above the bulb, held seven penny-weights and fifteen grains of mercury; and the whole gage contained fifteen ounces, eighteen penny-weights, and fifteen grains: having boiled the mercury in it, but
without

without box-wood or sheep-skin, on exhaustion it gave two hundred and fifty-four times rarefaction; and the upright boiled gage was brought down to three-tenths of an inch.

EXPERIMENT XII.

The same gage not boiled, with a piece of sheep-skin, which weighed one hundred and twelve grains, included in the receiver, now gave fifteen thousand, two hundred, and ninety-four times rarefaction: and the upright gage was three-tenths of an inch as before.

EXPERIMENT XIII.

The same gage again, not boiled, and without the sheep-skin, gave seventy-eight times; the upright gage being brought down to five-tenths of an inch.

EXPERIMENT XIV.

The same pear-gage with a piece of sheep's-leather, gave three hundred and sixty-four times; and the upright gage was at five-tenths of an inch, as in the last experiment.

EXPERIMENT

EXPERIMENT XV.

The last gage again, without the sheep's leather, gave forty-five times; the upright gage being brought down only to eight-tenths of an inch.

EXPERIMENT XVI.

The former gage again, with the piece of sheep's leather, gave sixty-six times; when the upright gage was at eight-tenths.

EXPERIMENT XVII.

With another pear-gage, the tube part of which was five and a half inches above the bulb, three inches of it held six penny-weights and twenty-one grains, and the whole gage contained seven ounces, nineteen penny-weights, and twenty-two grains. This gage being filled, and the mercury boiled in it, without the sheep's skin, gave one hundred and ninety times. The upright gage was brought down to three-tenths of an inch.

X EXPERIMENT

EXPERIMENT XVIII.

The last gage, not boiled, and with the former piece of sheep's skin, the upright gage being brought down to three-tenths of an inch, the former gave fourteen thousand times rarefaction.

EXPERIMENT XIX.

Bringing the upright gage down to five-tenths, the last pear-gage unboiled, and without the sheep's skin, gave sixty-nine times rarefaction.

EXPERIMENT XX.

Bringing the upright gage down to five-tenths, the last pear-gage, with the sheep's skin, and unboiled, now gave one hundred and ninety-one times rarefaction.

EXPERIMENT XXI.

The last pear-gage again, unboiled and without the sheep's skin, gave only thirty-nine times, when the upright gage was brought

brought down to only eight-tenths of an inch.

EXPERIMENT XXII.

The same gage again, unboiled, and with the piece of sheep's skin, the upright gage being at eight-tenths, gave fifty-seven times rarefaction.

In all these experiments, the pump was used dry, and the receivers were cemented down; and in all the first twelve experiments, the upright boiled gage was brought down to three-tenths of an inch above the surface of the mercury in the cistern, as it was in experiments the sixteenth and seventeenth. And by both these last gages it seems very plain, that the vapour, &c. had a considerable effect at a much less degree of exhaustion than what I used with the box-wood, by which I found no effect. Perhaps Mr. Nairne's box-wood was not so dry as mine; and I did not try it green, where the effect might have plainly appeared. Now all these experiments

seem fully to prove what has already been observed, namely, that this coating, or covering with damp, &c. takes place most where the air is highly rarefied. [See page 149.] And in all these experiments the degree of exhaustion is taken by the pear-gage in an erect position, of which notice has already been given. [See page 120.] These experiments also further prove, that a common constructed air pump may be made to rarify as much as one of the Smeatonian construction.

Mr. Nairne tells Mr. Bewly, November 22, 1776, that the pump with which these experiments were made, is different from Mr. Smeaton's, 'and as a proof that it is not liable 'to be out of order, I need only mention that 'it has been worked almost daily and hourly 'since the 21st of October till this day, and 'has not had a single drop of oil more than 'what was put to it when it was first put 'together, and at present I do not find but 'what it performs all its functions as well as 'at

‘at first*.’ Hence those experiments apparently were begun with this pump, at about October 21, 1776, but I knew nothing of what had happened any where, except what I myself was doing, till the 7th preceeding: so that I think my discoveries, relative to the fallacy of the Smeatonian pump and gage, stand quite independent of every thing else, any where, or at any time; and that all Mr. Nairne’s experiments, except what was shewn in April 1776, were posterior to my discoveries relative to the pump and gage. But the real existence of the vapour, by boiling the pear-gages, I did not discover till afterwards.

- It appears that many Gentlemen of the Royal Society were present with Mr. Nairne, when he shewed Mr. Smeaton the great difference between the two gages, &c. in April 1776, [see page 134] one of whom was the

* A strong proof of the excellency of the manner in which the workmanship was executed.

Hon.

Hon. Mr. Cavendish, whose great philosophical knowledge is well established. He then recollecting some former observations, very readily suggested an *hypothesis*, whereby to account for the difference between the two gages which he had just before seen. The substance of this *hypothesis* is given by Mr. Nairne in the Philosophical Transactions, but something different from the manner in which Mr. Smeaton gave it in his answer to my first letter to Dr. Watson, as will appear by and by.

On the third experiment in Mr. Nairne's accounts [see page 136,] a note is given, which says, 'It may be proper to take notice, that the 'pump, in every experiment hereafter-mentioned, was worked ten minutes, except where 'it is otherwise mentioned.' By this note it may appear, as if ten minutes were thought full long enough to work the pump. Hence it seems he had not tried what might be done by working the pump any longer time, or even supposed it necessary with the pear-gage:

gage; though it appears as if he did with the barometer gage. And I should in general have thought the same, for I do not remember that I worked my pump five minutes, nor above three, my receiver being small, [see page 116,] for any one of these experiments; and yet, I apparently produced as great a degree of rarefaction as any one. But in the succeeding experiments his ingenuity led him to see what would happen by working the pump half the time, that is, five minutes, and then pushing down one gage, purposely having two of them at the same time in the receiver; but stayed till the pump had been worked ten minutes before the other was pushed down, in order to see the difference produced by the difference of the time of working the pump. Thus when the first gage was pushed down in this experiment, no more air was supposed to get out of this gage; although the pump was kept working to ten minutes. Accordingly the first gage indicated but a low degree of exhaustion, to what it did in the former experiments, when the pump had been worked
ten

ten minutes. This difference, one would think, might have pointed out the trial of working the pump with a pear-gage, as much longer than common, as this last experiment was shorter; in order to see what difference that time of working the pump would make. I think it could have done no harm: and in particular, after what Mr. Nairne had represented to Mr. Bewly, December 19, 1776, when referring to the elastic vapour, (the idea of which the former informed the latter, he had from Mr. Cavendish) he says, 'The manner I account for these appearances is this: I imagine that there is contained in the leather, &c. an elastic fluid which is kept in a fixed, or condensed state by the pressure of the atmosphere, but when that pressure is in part taken from it, it turns into an elastic vapour, mixes with the dilated air and is exhausted with it out of the receiver. That when the pump has been kept continually exhausting for ten minutes, there then remains in the receiver and Smeatonian gage only a four thousandth part of air, the other being the elastic

'elastic vapour, which returns to its original
 'fluid state on being pressed by the atmos-
 'phere; in the same manner as steam when
 'turned into water, which, if I remember
 'right, takes up thirteen thousand times more
 'space when in steam than in water, and I do
 'imagine the longer the pump is kept exhaust-
 'ing that less air would be left in the Smea-
 'tonian gage.' However, this seems to have
 escaped all his careful variety of examina-
 tions; yet he might try it, and not finding it
 to answer, omit to insert it. But he found
 the barometer, or upright gage, the same at
 five minutes working, as it was at ten. If
 the pear-gage was not thus tried, perhaps
 what prevented it might be the discouragement
 given by Mr. Cavendish, that is, 'that
 'being continually generating and condensing
 'would never be exhausted,' as related in
 Mr. Smeaton's answer to my first letter, on
 this subject, to Dr. Watson, which, as I re-
 ceived it, appears as follows: 'Seeing these
 'experiments, at which were present Mr.
 'Cavendish, Mr. Aubert, and some other
 Y friends,

' friends, and its turning out as Mr. Nairne
' had said, Mr. Cavendish suggested, that
' under a certain degree of absence, of pressure,
' of atmospheric air, that the moisture in, and
' about the pump and glasses, raised a vapour
' from the watery particles that being conti-
' nually generating and condensing would
' never be exhausted, and which pressed upon
' the open leg of the syphon and raised the
' column in the other just as much as a light
' pressure of air; but as this watery vapour
' would even promote the escape of the at-
' mospheric air from the receiver, whenever
' the return of the atmosphere drove the mer-
' cury up into the pear-gage, the watery va-
' pour condensing to the side of the tube, left
' nothing but the proportion of atmospheric
' air, that under these circumstances remained
' to be unoccupied by the quick-silver; in con-
' firmation of this idea, he desired that the
' experiment might be tried, that all moisture
' might be avoided as much as possible; and
' as I remember, the syphon gage came much
' nearer to the level: and, as I believe, the
pear-

‘*pear-gage* shewed a less rarefaction. But I
 ‘ was the less exact, in noting the particulars,
 ‘ as I intimated to Mr. Cavendish my desire
 ‘ that he would give the society an account
 ‘ of the experiment with his explication upon
 ‘ it, which appeared to me equally new and
 ‘ curious, therefore, in hopes that Mr. Ca-
 ‘ vendish would do it I rested the matter
 ‘ there.’

The former being Mr. Smeaton’s exhibition
 of Mr. Cavendish’s hypothesis, that in the
 Philosophical Transactions now follows. After
 seeing the gages differ so much from one
 another, Mr. Nairne says, ‘ Mr. Cavendish
 ‘ accounted for it in the following manner.
 ‘ It appeared, he said, from some experiments
 ‘ of his father’s, Lord Charles Cavendish, that
 ‘ water, whenever the pressure of the atmos-
 ‘ phere on it is diminished to a certain de-
 ‘ gree, is immediately turned into vapour,
 ‘ and is as immediately turned back again
 ‘ into water on restoring the pressure. This
 ‘ degree of pressure is different according to

the heat of the water: when the heat is 72° of Fahrenheit's scale, it turns into vapour as soon as the pressure is no greater than that of three quarters of an inch of quicksilver, or about one-fortieth of the usual pressure of the atmosphere; but when the heat is only 41° , the pressure must be reduced to that of a quarter of an inch of quicksilver before the water turns into vapour. It is true, that water exposed to the open air will evaporate at any heat, and with any pressure of the atmosphere; but that evaporation is entirely owing to the action of the air upon it: whereas the evaporation here spoken of is performed without any assistance from the air. Hence it follows, that when the receiver is exhausted to the abovementioned degree, the moisture adhering to the different parts of the machine will turn into vapour and supply the place of the air, which is continually drawn away by the working of the pump, so that the fluid in the pear-gage, as well as that in the receiver, will consist in good measure of vapour. Now letting the
air

‘ air into the receiver, all the vapour within
‘ the pear-gage will be reduced to water, and
‘ only the real air will remain uncondensed;
‘ consequently the pear-gage shews only how
‘ much real air is left in the receiver, and not
‘ how much the pressure or spring of the
‘ included fluid is diminished, whereas the
‘ common gages shew how much the pressure
‘ of the included fluid is diminished, and
‘ that equally, whether it consists of air or
‘ of vapour.

‘ Mr. Cavendish having explained so satis-
‘ factorily the cause of the disagreement be-
‘ tween the two gages, I considered, that, if
‘ I were to avoid moisture as much as pos-
‘ sible, the two gages should nearly agree:
‘ this induced me to make the following ex-
‘ periments.’

Hence it appears as if Mr. Nairne did not
recollect Mr. Cavendish’s desiring all mois-
ture to be avoided, and the experiment to
be tried, by saying, ‘ I considered that if I
were

were to avoid moisture,' &c. any more than Mr. Smeaton did Mr. Cavendish's description of the necessary heat of water to evaporate in an exhausted receiver.

The idea of the elastic vapour seems to be allowed on all sides, as Mr. Cavendish's own: and were it not for the principle on which the idea itself is founded carrying with it such an insurmountable obstacle, the idea would not be so easily shaken; but as it is rested upon a supposition that on the admission of common air to it, or on the admission of it to common air, the effect being the same in both, it becomes of no consequence, or returns to its original state; thus the hypothesis seems wholly to destroy itself. He says, 'but
' as this watery vapour would even promote
' the escape of the atmospheric air from the
' receiver,' &c. or as Mr. Nairne relates, in the Philosophical Transactions, 'that water,
' whenever the pressure of the atmosphere on
' it is diminished to a certain degree, is immediately
' turned into vapour, and is as immediately

‘mediately turned back again into water on
‘restoring the pressure: or, that, it is conti-
‘nually drawn away by the working of the
‘pump, or, as he said to Mr. Bewly, that
‘it mixes with the dilated air and is exhausted
‘with it.’ On these premises, it seems al-
most impossible that this vapour should by
any means promote the escape of the atmos-
pheric air: for if it should be able to assist
the rarefied air to lift up any valve, (and in
particular, one whose resistance is equal to Mr.
Smeaton’s) in the middle part of it, yet when
it gets to the limits of its confinement, that is,
at the edges of the valve, it is instantly driven
back whence it came: seemingly as if the
common air stood to watch it with a sure
weapon, ready to demolish it at its attempting to
make its escape. Or it might be represented
more familiarly, by supposing a shower of
rain and hail falling upon a large plate of
glass, or the like, on which the shower
would fall, rain and hail mixed together; but
the hail would by the fall be made to bolt
back, or up again; and although the water
might

might lie lurking ready to pass, yet it could never get through the glass, any more than either the dilated air, or the vapour under the receiver, could escape at the valve.

In Mr. Cavendish's hypothesis, he says, as related by Mr. Smeaton, 'the watery vapour
'condensing to the side of the tube left no-
'thing but the proportion of atmospheric air,
'that under these circumstances remained to
'be unoccupied by the quicksilver.' I think he must consider the vapour to condense, and lodge perfectly smooth, contrary to the nature of vapour, damp, &c. on the inner surface of the tube, &c. and to leave no lodgements, or cavities, for any of the air to lurk in, as the mercury rises in the gage. But there is not a single hint in the hypothesis, or the experiments, towards making the vapour appear, in any shape, to any of the senses; and it is much to be doubted whether any of either vapour, or air, enters the tube more than what is perpendicularly under the column contained in the tube-part of the gage, agreeably

ably to the rules of hydrostatics, namely, that fluids press equally in all directions: whereas the boiled gages make it plainly appear; and also, if they are examined with a magnifier, that there are innumerable cavities, &c. as already observed, for the air to lurk in, all the way up in the bulb, be it ever so large, as well as the tube of the gage: and mostly the larger the bulb is, and the smaller and shorter the tube is, the greater is the deception, if the unoccupied part of the gage is taken in an erect position. But Mr. Nairne does not mention how he took the unoccupied part of his gages, whether in an erect, or horizontal position; which makes a very great difference. [See page 120.]

In the Philosophical Transactions, page 623, it is said: ‘ Now letting the air into the receiver, all the vapour within the pear-gage will be reduced to water, and only the real air will remain uncondensed; consequently, the pear-gage shews only how much real air

Z

‘ is

'is left in the receiver*.' But the pear-gage no more shews how much real air is left in the receiver, than what is left in the gage itself; on account of the lodgments and cavities, formed by the damp or vapour, on the sides of the glasses, &c. nor do those experiments make it appear that the vapour is reduced to water.

Mr. Nairne says, 'The other being the 'elastic vapour, which returns to its original 'fluid state on being pressed by the atmosphere; 'in the same manner as steam when turned into 'water,' &c. [See pages 161 and 162.] But this is something contrary to most of the other conceptions of this vapour; which suppose that it is changed immediately into water, or its original state: whereas steam seems to remain some considerable time before it is perfectly turned into water, or back again into its original state.

* The same is maintained also in Philosophical Transactions, pages 636 and 637.

On

On the first of May, 1777, Mr. Bewly wrote to me as follows: 'In a letter which I received this morning from Mr. Nairne, whom I had informed of some of the particulars of your experiments, he says, there is certainly something or another that Mr. Brook does not attend to; otherwise there would not be that difference in his gages:' and then gives some examples with his gages, adding, that 'they always agree, I never find my gages differ. He suspects too, as I did, that you have not attended to the *time*. The time of working the pump is certainly a very material consideration; if the hypothesis of an elastic vapour be true.' In reading Mr. Nairne's account in the Philosophical Transactions, I was very glad to see, that he afterwards found a difference in his gages, as well as I had done in mine*. And in respect to attending to time, all my guide was the bringing the boiled upright gage as low as I could; which I believe never exceeded three minutes,

* See Philosophical Transactions, pages 626 and 627.

and often not so long; and yet my apparent powers, or degrees, or number of times rarefying, were equal to any, as mentioned just before: so that the *time* does not appear to be of so material a consideration as was believed, whether the truth of the hypothesis be admitted or not.

Having used my pump indifferently with wet leathers, &c. and having informed Mr. Bewly, that the effect went off in six or eight hours, and sometimes much sooner, by working it often in the time; and sending accounts of some experiments, to him, made with the pump in that state, and from the intercourse between himself and Mr. Nairne, the latter advised the former not to use any water or leathers soaked in water, for he must never expect his pump to exhaust after he had used water, any thing like so near as it did before. This being contrary to what I had told Mr. Bewly, it added much to my satisfaction in reading Mr. Nairne's account in the *Philosophical Transactions*, to find, that
my

my former observations to Mr. Bewly were so strongly verified*.

Mr. Nairne, having been describing the construction of his long and short upright gages, says, 'gages made with these precautions seem to me to be the most to be depended upon, in determining the actual diminution of the pressure on the surface of the quicksilver in the cistern†.' And it may be readily supposed, that this sort of gages will be the most to be depended upon in every pneumatical operation relative to rarefaction or condensation. For the pear-gages shew nothing at all till after the designed exhaustion is over; whereas the other sorts do it continually from the least to the greatest degree: and even after the designed exhaustion is over, the pear-gage shews no more than the other would under the same circumstances, that is, by being boiled; namely, that there is a vapour, &c. that arises and takes place on the sides of what-

* See Philosophical Transactions, 642. † Ib. page 640.

ever is within the receiver, at a considerable diminution of atmospherical pressure: and this verifies that part of Mr. Cavendish's hypothesis in a very high degree.

After finding Mr. Nairne's comparison of the two pumps inserted in the Philosophical Transactions; as I knew him to be as great a stranger to secrets as Dr. Priestley is to persisting in a mistake; the next time I went to London I took one of my boiled upright gages with me, intending to ask his leave to let me try one of his best Smeatonian pumps with my gage, which he seemed with great pleasure to grant: and he having just before finished a new pump on that principle, with some additions and improvements, he readily gave me leave to try what I liked with it, and desired his foreman to assist me. But when we had put my gage on his pump, and exhausted it, he seemed to be something disappointed, to find that his pump would not bring my gage any thing like so low as he expected. We repeated our trials, but the result

fault was the same: and I should have been equally disappointed had his case been mine; though he said but little about it: nor could we with that pump, which was finished in the best manner, bring my gage so low as I could with these pumps which I myself had made, by full one-tenth of an inch: and I could only suppose it to be owing to the difference of boiling the gages: which, to me, soon appeared very plainly to be the case; though Mr. Nairne had conceived by verbal description that my method of boiling them could not be so perfect as his, on account of the position in which I held my gages, while they were boiling. Accordingly he desired his boiling apparatus to be got ready, in order to let me see his method of boiling; which was done: and I could do no less than offer to shew him my method, which was likewise done one night after we had been at the meeting of the Royal Society, where he was so obliging as to ask me to go with him. And as I was boiling a barometer tube, he seemed satisfied that my method of
boiling

boiling was not inferior to his, in regard to perfection*; though I found that he could boil several to my one: but it appeared to me that his method was scarcely boiling at all, or little more than heating them pretty hot: nor did it appear to me that he could boil a pear-gage at all. Some little time after my return home I received a letter from Mr. Nairne, in which he informed me, that they had discovered that some of the shoulders of the pump which I tried at his house, were not screwed so close as they should have been; which was the reason why the pump would not exhaust any further. I gave him for answer, that if the shoulders were not screwed close, the pump would not have been airtight; but in that respect it appeared to be very perfect; therefore that could not be the

* Notwithstanding all this, the next time I saw Mr. Nairne, he had, amidst his multiplicity of business, quite forgot my boiling a barometer tube for him, though some of his workmen, who were present, remembered it very well: also he did not remember my sending him a boiled gage for an air pump, from Norwich, which I had also done.

reason

reason why it did not exhaust any further. Here seems to be a more full demonstration, that the common constructed pump may be made to exhaust equal to the Smeatonian one.

From the experiments related in Mr. Nairne's accounts in the Philosophical Transactions, which appear to have been conducted with a great deal of ingenuity and care, together with those herein related, I think it plainly appears, that the supposition relative to the excellency of the Smeatonian constructed air pump, arose entirely from the fallaciousness of the pear-gage; and that the common constructed pump, as well executed as Mr. Nairne does his, will exhaust as much as any hitherto constructed*.

Some time towards the latter end of April 1777, I received a letter, dated only *midnight*,

* Mr. Nairne informed me that he has lately constructed an air pump so as to work without a valve in order to exhaust further, but I do not remember that he told me how much farther than his former pumps it would exhaust.

A a

from

from Mr. Bewly, inclosed with a latin pamphlet, which pamphlet was sent hither to ask the assistance of Dr. Manning, in a phrase or two, which the Doctor readily gave him in a note concerning the pamphlet, with a postscript; and sent it to me to forward to Mr. Bewly; which I did May 10, 1777. The following is the substance of the postscript. ‘ Would
‘ Dr. Anybody, or Mr. Anybody, at London,
‘ or any where, have supposed that the Smea-
‘ tonian pump would do no more than any
‘ other good pump; which was Mr. Brook’s
‘ first discovery; and also that the Smeatonian
‘ gage was not an accurate one, or at least, that
‘ it varied so much from the other, and that it
‘ also varied so much in itself at different times,
‘ before his experiments led the way?” but
Mr. Bewly gave no answer to the postscript, although he wrote to me various times very soon: on which Dr. Manning gave me another note to enclose in a letter which I sent to Mr. Bewly, June 2, 1777. I transcribed the note, and the following is a copy of it. ‘ Dr.
‘ Manning’s compliments to Mr. Brook, and
‘ thanks

' thanks him for the perusal of Mr. Bewly's
 ' letter, he is much disposed to be pleased
 ' with any thing of his, as he has formed a
 ' good opinion of both his head and heart;
 ' but pray inform him that he does not seem
 ' to apprehend the meaning of his question
 ' added to your former letter, I will therefore
 ' intimate what it meant, namely, in plain Eng-
 ' lish, that the whole course of experiments
 ' relative to the two gages and the present
 ' comparison of the common pump and that
 ' of Mr. Smeaton's, took their rise from your
 ' experiments communicated to Mr. Bewly
 ' and Dr. Watfon.' So that it seems to me,
 that I am not the only one who thinks,
 if any claim is to be laid to the discovery of
 the fallacy of either pump or gage, that I
 have the greatest.

As observed before, Mr. Smeaton describes
 his improvements, if they were such, on the air
 pump, in the XLVIIth Volume of the Philoso-
 phical Transactions; and he describes two ad-
 vantages in one paragraph, page 420, which

runs thus: ' Another advantage of this construction is, that though the pump is composed of a single barrel, yet the pressure of the outward air being taken off by the upper plate, the piston is worked with more ease than the common pumps with two barrels: and not only so, but when a considerable degree of rarefaction is desired, it will do it quicker; for the terms of the series expressing the quantity of air taken away at each stroke do not diminish so fast, as the series answering to the common one.

The former of these two advantages is attended with two notes; and seems to want more, to enable it to keep firm on its basis: the former is as follows:

' It is obvious that these improvements will equally obtain, whether the pump is constructed with a single or double barrel.' And the latter stands thus:

' Because,

‘ Because, though the pressure of a column of air, equal to the diameter of the piston rod, still presses upon it, yet, as there is only the friction of one piston, and that not loaded with the weight of the atmosphere, the friction of the leather against the side of the barrel, and that of the rack and wheel, is much less: so that notwithstanding the addition of friction in the collar of leathers, that of the whole will be less.’

In the double barrelled pump, the pistons are connected together by the racks and wheel; therefore, whatever is lifted up by one piston is pressed down by the other, exclusive of the friction; and though there is the friction of the leathers in both barrels, this has likewise the friction of the piston against the barrel, and the friction of the rod in the collar of leathers: and though the rod is less in diameter than either of the pistons in the double barrelled pump; yet the Smeatonian piston is much larger, otherwise one of the supposed advantages is lost. And although it
is

is said that *the piston is worked with more ease than the common pumps with two barrels*; yet I cannot find how it can work with so little ease, if they are well executed: but perhaps Mr. Smeaton here means by *common pumps*, those of an inferior sort of workmanship; if not, then I think the principle is much in behalf of the common pumps.

The other advantage, which is, to rarefy quicker, is left without any assistance; therefore, is almost ready to fall of itself. For with two barrels, their size need not be over large, to be equal in capacity to the Smeatonian single barrel, whatever they may exceed it. And as the wheel in the latter is usually made smaller, and the winch longer, in order to make the labour less, than in the former, and the barrel considerably longer; consequently the hand has to pass through a much greater space. I think that, upon an average, four dips, or strokes, with the two pistons, may be made, in the same time, or perhaps sooner, than *one*, in the Smeatonian construction. And if
one

one barrel in the double construction should take away only half the quantity of air at each dip of each piston, that the single barrelled construction will do; then if only two strokes (namely, one stroke with each piston) with the former, can be made in the same time that one stroke can be made with the latter, I think the quickness of exhausting is nearly equal; or if a greater number of strokes can be made with the former, in the same time, than one in the latter, I think the common pumps, like that which I saw, which came from Mr. Nairne, particularly with a small alteration* in the piston, are very much preferable. Consequently this destroys what is advanced in the second note.

The first note says, 'it is obvious,' &c. Therefore of what use is the note, as it illustrates nothing? for the improvements, if they

* This alteration I think I could plainly shew to any body, particularly to a workman: but I cannot well do it in writing, without a copper-plate.

were

were such, which the note refers to, seem to be described in a preceeding paragraph which is as follows: 'Hence, as the piston may be made to fit as near to the top of the cylinder, as it can to the bottom; the air may be rarefied as much above the piston as it could before have been in the receiver.' But the improvement referred to seems to be of no use, namely, a method to assist the valve to rise easier. For I believe it will readily be allowed, that, be the construction what it will, there must always be air enough left to raise the valve; also, that this will be the case no longer than a more perfect vacuum can be made in the barrel below the piston, than there is in the receiver on the pump, be their capacities little or great: and also, that this will be the case, whether, or not, the piston works in a partial vacuum; for as the piston in a common pump can be made to fit at the bottom as close as it does in a single barrel pump; (and I think they may with care be either of them made to fit as close as any other substances, or surfaces, even so as to

to exclude all air between them, as to sense) then, whether the barrel works in a partial vacuum, or not, the space under the piston will be as perfect a vacuum, as the materials will admit; unless any air can get into that space out of the receiver: which will depend chiefly on the ease with which the valves can be made to open and shut, and the Smeatonian upper valve is equally exposed to the pressure of the common air, as the upper valves in a common double barrelled pump. Therefore the supposed use of the note, as well as the improvement it refers to, does not clearly appear. Nevertheless, the plausibility, with which the whole is related, is excellent, and wants no assistance whatever. Probably, this brought on that strength of expression by Mr. Bewly. [see page 127.] However, exclusive of all these plausibilities, and supposed excellencies, the principle itself, on which they are built, runs diametrically opposite to that most perfect and unerring rule in mechanics, as well as throughout all philosophy, that whatever is gained in power is lost in time.

CHAPTER V.

OBSERVATIONS

ON THE

BAROMETER.

WHOEVER observes the Barometer attentively, with respect to the alterations of the weather, relative to wet and dry, will find, that though its indications are not always certain, yet on the whole it is mostly so, or, that it is the best director we have. The theory of it being very well understood, I shall omit it, and chiefly attend to the manner of making that instrument.

A variety of incidents following each other brought on my making barometers. It al-

B b 2

ready

ready appears what occasioned the discovery of the fallacy of the Smeatonian pump and gage: one circumstance was the enquiry of various persons about them, one of whom was Mr. J. Ferguson, F. R. S. who, as already observed, gave a course of lectures in Norwich, in the year 1769, and the luminous flask in his lecture on electricity was the first I ever saw; and though I had no air pump to do it as he did, he told me how to make one without a pump; which was, by joining a tube to the mouth of a flask, then filling both with quicksilver, and inverting them in the Torricellian way, and sealing the mouth of the flask before I separated them. My method of joining the tube to the flask was by the help of cement: but by the warmth of cementing the flask and tube together, previous to their being filled, I found much damp in the flask when I had separated it from the tube. I afterward tried to disperse the aforesaid damp in the flask, by warming it before separation; but found no advantage. This put me upon trying a thin glass tube in the Torricellian method;

thod; but, when it was filled with mercury, I found it was not so clean as it should have been; or, it appeared as if wet withinside: and having but a small stock of tubes, I was the more anxious about preserving them: but by having Mr. Beighton's account of Mr. Orme's manner of managing them (which was also merely incidental as will soon appear, and of which notice will be taken in the sequel) I tried to disperse the moisture by heating the tube with the mercury in it before a common fire; which soon turned the moisture into large bubbles of air: thus I was encouraged to try to get them out of the tube: but they chiefly vanished when they came where the tube and mercury were cold.

My next apparent resource was, first, to drive away all the moisture from the sealed end of the tube, and then to make its whole length pass the heat in succession: which answered very well, and left the mercury very bright in the tube.

Finding

Finding the advantage of this last method so plainly to appear, a more convenient method to heat them soon occurred, and was easily put in practice. Thus, by the time that I had done three or four tubes, I found that I made them as perfect as I could make them at all. But though this method assisted me considerably afterward, I was much disappointed in my present pursuit, namely, of making the exhausted flask for electricity: for let the tube be of any length it would above the mercury as a barometer, I could not make any electrical light appear in the void. Soon after this, I finished my air pump, already mentioned; with which I reduced the column of mercury to half an inch: but the effect was the same. So that I got no assistance from heating, for making luminous flasks: nor did I now want it. And as the aforesaid method of heating is what I still prefer, I shall briefly describe it. I procured two sheets of tin from the tin-plate workers (iron is better, as will appear by and by) bent to a right angle in the middle of it the longest way, and
riveted

riveted together, one at the end of the other. A partition of the same is fixed about ten inches from one end, with a part cut out from its upper side to within a little more than an inch of its bottom, and about three-fourths of an inch wide. The whole is to be placed on a stand of a convenient height, on a declivity, making an angle of about ten degrees with the horizon, with that end uppermost towards which the aforesaid partition is fixed: then above this partition, lay some baker's coals (in preference to charcoal) close to the partition, in order to be kindled; of which two or three handfuls are sufficient for one tube.

After this preparation, take a glass tube not less than three feet long, with a bore about three or four twentieths of an inch in diameter, but not more (as will appear afterward) and the tube to be nearly as thick on all sides as the diameter of the bore, that it may be sufficiently strong. Let this tube be nicely sealed, or closed, at one end, and let it be as
clean

clean as possible: then fill it with mercury as pure as possible; when the coals are kindled in the caldron, or fire-holder, lay the sealed end of the tube, with the mercury in it, into the place cut out of the partition, upon the lighted coals: if the fire be slack, blow it a little with the mouth, or the like; let the tube lie amongst the lighted coals, till the mercury in it boil pretty strongly. When the first part of the tube appears to be sufficiently boiled, move it onward, by little and little, and so on the whole length of the tube.

After the tube is filled in the common way, before it is put upon the fire, take out about three inches of the mercury at its open end, in order for room, that the mercury may play up and down as it boils, without being lost. Thus, by taking these methods, I find no inconvenience arising from the heat of the tube in holding it whilst it is boiling; for, by the time that the sealed end has passed the fire, before the other comes to be so near it as to be too hot to be held in the hand, the former will

will be cold enough to hold the tube by, while the remaining part of it goes on. [See Monthly Review, Vol. XLIX, page 584.]

Tubes for barometers prepared in this way, I find generally, will support a column of mercury of the same height: or, that instead of a variation of half an inch, as sometimes happens with those made in the common way, the former will very seldom vary half a tenth of an inch. Also, if they are well boiled, the mercury will always be suspended to the top of the tube, and will not fall down to its proper height, till it be shaken pretty strongly to bring it down; and sometimes I have had them so strongly suspended, that after the barometer has been sent many miles, it has been returned as useless. One instance of this kind happened at the time I was writing this article.

The suspension of the mercury to the top of the tube occurred in the very first tube that I heated before the common fire, though the

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adhesion

adhesion was weak; but the more perfect suspension did not occur till afterward; which seems to be owing wholly to a more perfect exclusion of the air, damp, &c. from within the tube: whence the adhesion appears to be stronger between the glass and the mercury, than it is between the particles of mercury themselves; for the mercury will frequently separate at an inch or more from the top of the tube, and fall down to its proper height, while the upper part of the column will remain suspended where it was. Then, by inclining the tube, the mercury will join, and sometimes only a part of the column that remained will come down, and a shorter part of it will still remain suspended; which is brought down likewise by inclining the tube; and sometimes this will happen months after the barometer has been finished and fixed up.

When the tubes are completely boiled, they may be hung up with their sealed end downwards, till they are wanted, when they may be

be cut off to their proper length, with a file, in which I never exceed thirty-two inches and a quarter, (preferring the upright barometer) and then they may be fitted up in whatever manner may be thought most proper, either for ornament as well as use, or for use only, either in a portable way or not. But if they are to be fitted up in a portable way, I always preserve a hole through which the air may circulate into the cistern, which is opened or shut by means of a screw, and is equally good as if it were open all round.

Not long before the time that the foregoing observations occurred, it happened, that I, professionally, purchased a collection of books, &c. amongst which was No. 448, of the Philosophical Transactions, which contains the account of Mr. Orme's improvements of the barometer, given by Henry Beighton, F. R. S. which stands as follows.

‘ Mr. Charles Orme’s improvements of the
‘ barometer, by the method following:

‘ First, the *quicksilver* is all purified from
‘ its dross and earthy particles by distillation;
‘ and when the *tube* is filled by a pound and
‘ half, two or three pounds of *mercury*, and
‘ all the *air* got out by the methods used in
‘ filling *tubes*, then the remaining *air* is got out
‘ by such an *intense heat of fire* as makes the
‘ *mercury* boil; by which ebullition an innu-
‘ merable quantity of small particles are emit-
‘ ted, and blow with a great velocity at the
‘ open end of the *tube*, till all the *air* is quite
‘ cleared out; which curious as well as fa-
‘ tiguig operation is continued for the space
‘ of four hours: and when no more bubbles
‘ would rise in the *tube*, it remained whole,
‘ with its *mercury* of a most lively sparkling
‘ brightness, with this difference only, that
‘ the *mercury*, so purged from its air, did not
‘ fill the *tube* so high as when first put in by
‘ about two *inches*; which is a plain demon-
‘ stration, that in that *tube*, which was 49
‘ inches

inches long, there was interspersed in the mercury at first filling it, so much air as would fill two inches of the said tube, which was a 24th part of the said space.

The whole operation I myself attended the 20th of January, 1734-5.

And further I can affirm, that every part of the mercury boiled for a long time, and the tube was gradatim so red hot, that with a warm knife I could make impressions in any part of it.

And this I the rather mention, by reason I have heard several persons, and those not incurious, affirm it was impossible.

And that this is the most sure and certain (if not the only) method for getting out all the air, may be judged by the boiling of water, which in its ebullition does emit a great quantity of air for a long space of time.

We

We are none of us at present, that I know of, so fortunate as to be master of the art of bending glass tubes, or making impressions on them, when they are filled with mercury, by any means whatever. I have often, in vain, wished so to do: which makes me ready to think, that Mr. Beighton must, some way or other, have been as much mistaken in this, as Dr. Priestley found himself to be, relative to the Smeatonian pump or gage, already observed. And I must own that I should have been much more ready to believe it, were it not, that all my own observations in boiling tubes or the like, directly contradict the fact. I will here relate a case which happened before I had made myself so much acquainted with boiling tubes, &c. as I was afterwards.

Having formed a plan to make a barometer in an inverted position, in order to get it more out of the way of accidents, I made a bulb sufficient to hold about a pound of mercury, at the end of a barometer tube
of

of a sufficient length, to be turned up at the lower end, syphon-wise, that the rise and fall of the barometer might appear in the turned-up part, which inverts the scale; and the bulb at the top to answer the end of a cistern. I have always found barometers made in this way to support the longest column between their upper and lower surfaces: and as the fact is a sufficient proof, I omit the theory at present.

I say, I have found barometers made in this way to support the longest column, when boiled; but boiling them in this shape was not so easy at first. When the bulb and its tube were filled with mercury, in order to be boiled, I put the bulb in a hollow place amidst the coals, with the barometer in an erect position, in order that the bubbles might rise into the tube, and afterward be driven out by boiling in my common position. But, in my haste, I did not consider the pressure of the whole column of mercury on the bulb, and that it would retard the appearance of
its

its boiling; so that I heated it very much, even so hot, that the bulb was softened enough to give way: but the mercury being so much pressed and heated, as soon as the glass gave way, the mercury contained in the bulb instantly turned into vapour and dispersed entirely, and only what was in the tube ran down into and through the fire, which was very fleet. So that I cannot conclude upon Mr. Beighton's description of Mr. Orme's method any otherwise, than that he must have been very much mistaken, or knew very little about it. Besides, whenever I distill mercury, I always do it in a glass vessel, but never find the glass to be at all softened: and when I boil the mercury in the tubes, either thick or thin, they appear to have no softness or tendency to bend in the least.

And I must own, that I never found any such diminution in the length of the mercury in the tube after boiling, as is above described; provided that I took care to lose none of it out of the tube while it was boiling

ing: which makes me think, that there must have been somewhat else besides the explosion of the air that the mercury contained, which was the cause of the tube not being filled so high as when the mercury was first put in by about two inches, or a twenty-fourth part of the space; nor did I ever find any *blowing* at all at the open end of the tubes, though I have occasionally boiled tubes much smaller and much larger (as will appear very soon) than Mr. Orme's appear to be, where ever they are seen: so that I cannot think the demonstration of either is plain enough to be depended upon.

Mr. Beighton, speaking of the boiling, &c. says, 'which curious as well as fatiguing operation is continued for the space of four hours: and when no more bubbles would rise in the tube it remained whole,' &c. I am sure that if the heat were continued four hours more, if there were mercury enough remaining unevaporated in the tube, the rising of the bubbles would likewise be continued: my tubes

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never

never take up more time, to boil one of them, than three-fourths of an hour, and often not so much. Therefore I must conclude, that if Mr. Beighton had been as much acquainted with the boiling of mercury in tubes of glass, or the operations carried on in a laboratory, as he was with those carried on in the clouds and the atmosphere, he would never have written his paper in the manner it now appears.

Amongst the aforefaid collection of books, &c. was a pamphlet written by E. Saul, A. M. which in many parts is very curious and exact, but not altogether so throughout. In this work, pages 51 and 95, Mr. Saul recommends the sloping barometer. In page 101, for an upright barometer, he recommends a tube of nearly half an inch diameter (it must be supposed that he means the bore of the tube) in order to judge of the convexity or concavity of the surface of the mercury in the tube occasioned by the alteration of the pressure of the atmosphere on the mercury in the cistern; and

and that the mercury will rise and fall sooner than in a smaller bore ; Mr. Saul here says, ‘ if the *mercury* appears to be concave, or depressed more in the middle, than at the sides of the tube,’ &c. But it is certain that if a tube be clean within, the mercury will never appear concave on the surface, be the tube great or small, and in particular at its sides. Let any common *clean* phial, of any size whatever, be partly filled with pure mercury ; the sides will always appear convex and the middle not depressed, even if a syphon be applied in the middle of it to decant it out of the phial : all the time that the surface of the mercury is sinking, its sides will have a convex appearance ; but if the phial be moist, or the mercury foul, the surface of the latter will not be the same : it is only where purity is wanting, that concavity takes its place with respect to mercury itself in these cases.

At the end of the XLVIIth Volume of the Monthly Review is the annunciation of that almost inestimable work of Mr. De

D d 2

Luc's,

Luc's on the barometer, &c. which was about a year after I had acquired the method to boil tubes for barometers*, and I cannot but own that the manner of the finishing of the account in the Review concerning it rather prejudiced me against it. However, when the book came to me, I read it with as much pleasure as I ever had in reading any book whatever: and although he knew no more of what I was doing, than I knew of what he was; if I had put down my own observations and description of the appearances and process of boiling tubes for barometers, they would have been as near to his as if we had had the most familiar intercourse.

The preference that is to be given to tubes whose bores are not very large and their sides pretty thick, arises chiefly from the danger that attends them in carriage or on any removal: besides, a tube of about two lines bore I find to be much easier, and sooner boiled, than

* See the note in Monthly Review, Vol. XLIX, page 585.

one whose bore is three or four lines in diameter: and the apparent specks of air are more easily kept from being left behind. But in tubes whose bores are large and their sides and tops thin, the motion of the mercury at any time very much endangers them. Mr. Nairne, knowing my method of boiling barometers, &c. sent two tubes, with large bores and thin sides, ready filled to me to boil for him; which I did, and returned them as safely packed as I could: but before he received them they were both broken to many pieces; consequently all the mercury as well as the tubes were lost and spoiled. But I never knew one such as I have already described, to break in carriage. When tubes are *well* boiled, they seem to me to be much more liable to be broken by the motion of the mercury, than tubes that have been but *moderately*, or not at all boiled. I say, they seem to me to be much more liable to be broken, for this reason, that in ^{un}boiled tubes, the lining, that Mr. De Luc very justly speaks of, greatly saves them, and softens the stroke of the mercury

mercury against the glass: for the stroke is manifestly smarter in tubes that have been well boiled, than it is in tubes that have not been boiled at all; owing to the air, or that lining, which nothing will displace but heat, being all dispersed, and the glass being left quite bare and naked: so that the stroke of the mercury meets with no resistance but exerts its force immediately on the glass; and the glass has no defence against the mercury.

Those apparent specks of air, just mentioned, though in boiling they have that appearance, yet they seem to be nothing but mercury in an evaporated state, or the vapour of mercury not perfectly condensed, till the tube grows cool, when they quite disappear: yet I do not like to leave them behind. When the mercury is boiling in the tube pretty strongly, there will often be a space of an inch or two, or more, in tubes, whose bore is rather small; which space at the same time is perfectly filled with vapour from the mercury; and appears, while hot, extremely elastic,

elastic, and diaphanous as air itself: and, if care be taken, when the fire is rather low, the upper side of the bore will soon become studded with particles of mercury condensed upon it: but while the vapour remains uncondensed, its elasticity appears to be very great. Mercury, when boiling, seems to turn much faster into vapour, than water when boiling into steam; and likewise turns very quickly into mercury again, when in a cool place, if care be taken to preserve it from escaping.

The thickness of the glass has been said to hazard the breaking of the tube at the time it is boiling. But if the bore be not less than one eighth of an inch, they may be very safely boiled, if the fire be small and clear: but if the fire be large or long, their preservation is very hazardous. A tube one eighth of an inch in the bore may very safely be boiled, let the thickness of the glass, moderately speaking, be what it will. A tube half an inch diameter divided into three parts,
two

two for the thickness of the glass on each side, and one for the bore, is as good as any, in my opinion.

If a tube break whilst it is boiling, great part of the mercury is liable to be much damaged, by amalgamating with the tin on the plates in which the fire is laid: but if, instead of sheets of tin, an iron one is at hand, the amalgamation is all avoided: yet if the mercury be fouled in this or any other way, by having tin, lead, or the like mixed with it; it is easily restored to its original purity by distillation, be their quantities what they will: but if the proportion of adulteration be rather small, the mercury is still more easily made as perfectly pure by agitation; a method discovered by that indefatigable philosopher Dr. Priestley; which method was much improved by Mr. Bewly, and afterward much more improved by the Rev. Mr. Morgan; and by which it is now become very useful*.

* The method I use for this purpose is, to boil the adulterated mercury in a common four ounce phial for the space of about a minute,

Whatever may be the case in general with barometers, those made after the preceding method (two of which I have had by me more than ten years) do not appear to be the least impaired for a long space of time, but sustain the same length of column as in one newly boiled: so that by this, as well as by the air pump, pure mercury does not appear

minute, in which time very little, or none, of the included mercury will evaporate out of the phial, or less time will do if the whole of the mercury boil perfectly in the phial, over some lighted coals from a baker's oven, being less offensive than charcoal: but a common kitchen fire will do as well, if it be not too fierce: then take it from the fire, and when it is cool, shake or agitate the mercury in the phial pretty strongly: and if the quantity of adulteration be not very great, a quarter of a pound of the adulterated mercury so boiled, and briskly agitated in the phial, will become perfectly pure in less than a quarter of an hour: but if the adulteration is very much, or if the quantity adulterated be a pound or more, in the former case the operation will succeed much better, by mixing some pure mercury unboiled with that which is not pure and has been boiled; and then churning or agitating them together: or in the latter, it will do quicker, by pouring about a quarter of a pound of it at a time, into another phial, and churning it in that, and so on till the whole is purified, putting the purified portions all together in a separate phial: when the fouled mercury

pear to imbibe atmospheric air, or but in an extremely small degree, and very different to what Mr. Beighton has represented it, if the vessel be clean in which the pure mercury is contained: but if the vessel be foul, the air which the foulness contains will make itself appear on the air pump; yet it cannot be said to come out of the mercury. One of the
aforesaid

mercury has been churned some time, instead of appearing nearly enough done, it looks much more foul, and the purification comes on almost all at once as to appearance; which may seem singular: and if the quantity of fouled mercury be a pound or two, a beginner in this way of purification may be discouraged by its not going on so fast as might be wished. In this case, divide the fouled mercury into smaller portions, as before observed. When the fouled mercury has been sufficiently agitated, and is become pure, the adulteration will turn black and adhere to the sides of the phial, or swim on the surface of the purified mercury; and they will not easily mix together again: then pour the purified mercury through a funnel made of common writing paper, with a small orifice at the bottom: the purified mercury will run through the funnel, and leave all the blackness or adulteration in the funnel, where it may be squeezed by the fingers; and force the remaining part of the mercury through the funnel: but the foulness will all remain behind, so that very little of the mercury need be wasted throughout the operation.

aforesaid barometers has very often been placed on an air pump; and the column of mercury in it brought down to within less than half an inch of the level with the mercury in its cistern, and as often let up again: still the mercury apparently strikes the top of the tube as smartly, as if it had never been brought down at all: and taking the whole time of the aforesaid, *more than ten years* past, the columns of mercury in these barometers have been, for more than two-thirds of the time, not less than thirty inches high between the surface of the mercury in the cisterns, and the surface at the tops of the columns in the tubes, and often-times very much higher, yet I have seen them down to 28, 35: and 30, 82 was the highest I ever saw them.

F I N I S.

The barometre is a glass tube, closed at the top, and filled with mercury, which is brought down to a certain height by the weight of the air. The height of the mercury is measured by a scale, and the difference between the height of the mercury and the height of the air is the barometrical height. The barometrical height is a measure of the pressure of the air, and is used to determine the height of a place above the sea level. The barometrical height is also used to determine the height of a mountain, and the height of a ship above the water. The barometrical height is a very useful instrument, and is used in many different ways.



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